INDO-EUROPEAN PREHISTORY

By John C. Kerns

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INDO-EUROPEAN PREHISTORY

1. FOREWORD

This book is a sequel to a pamphlet I wrote in 1966, and privately printed and distributed in early 1967, entitled "The Eurasiatic Pronouns and the Indo-Uralic Question". Lacking professional status in prehistory, including diachronic linguistics, and being preoccupied with a busy engineering career, I did not attempt to air the matter in official publications. However, Bjoern Collinder, the great Swedish linguist, has honored me several times by citing this pamphlet in the bibliography of current publications, most recently in the second edition of his seminal Fenno-Ugric Vocabulary (1955, 1977), the first in a trilogy of books in English on Uralic which also includes his Survey of the Uralic Languages (1957) and his Comparative Grammar of the Uralic Languages (1960). Therefore I feel an obligation to make the contents of the pamphlet more generally available to the public, at least in its essential features; hence the present book includes an edited and shortened reprint of it.

During the intervening 18 years, of course, new publications in prehistory and linguistics have continued to appear, and it is not surprising that some of my ideas and conclusions have changed in minor ways. For example, I believe that I now have a clearer understanding of the nature of the fan-like expansion of the IndoEuropeans from the upper bend of the Danube, in what is now Hungary, as evidenced by the remains of the Linear Pottery Culture (called the Danubian I Culture in my pamphlet in imitation of Gordon Childe). In general, my once major reliance on Childe has been supplemented by references to other, mostly later, prehistorians. The major benefit of this is a more precise knowledge of the dating (generally older) of significant developments in the affected regions.

I regret that I never got to meet Collinder. But in a short vacation visit to Uppsala in August 1959 I did manage to meet his younger colleague, Prof. Tryggve Skoeld, who was most generous with his time. He guided me to the pertinent works of Collinder as then available, and he honored me with long and most delightful discussions of IndoUralic and related problems. Needless to say, I am profoundly grateful.

Skoeld suggested that I write a paper on the IndoUralic problem, but at the time I was unable to take his suggestion seriously since I was not aware of anything I could say on the topic that had not already been said by others more competent than myself. It was not until several years later, while I was still engaged in engineering work, that an off-hand remark by Collinder in the Introduction to his Comparative Grammar began to germinate in my mind. Inter alia, he wrote "The Indo-Uralic hypothesis cannot be regarded as proved. The pronominal stems *show a striking conformity, and there are some resemblances in the derivative suffixes and flexional endings too; but there are very few common words."*

This was what inspired me to devise the statistical test described in my pamphlet. I wanted to know whether the "striking conformity" was real or whether it was merely a subjective figment of Collinder's imagination. I concluded that the conformity is indeed real, having an almost vanishing probability of being false. I then proceeded to consider the logical consequences of this result, concluding that the conformity cannot be explained unless we accept the IndoUralic hypothesis, the paucity of common words notwithstanding.

I wish to emphasize that my statistical analysis was never intended to be regarded as definitive. On the contrary, I have always hoped that other people would become sufficiently interested to make investigations of their own and draw their own conclusions. For this reason I have not undertaken to refine the analysis except to add a few supplemental comments herein -- I am well aware that it can be improved.

In the course of this study I have become increasingly aware of the fact that the IndoUralic connection is only a part of a network of similar links among several other linguistic families, which together exist as members of a parent group which we call Nostratic. The present book includes an attempt to clarify some of the relationships within that larger group, with particular reference to the prehistory of IndoEuropean and its speakers. To the extent feasible, I have tried to correlate archeological phenomena along with linguistic phenomena in order to suggest a realistic pattern of events and developments.

Aside from the edited and shortened reprint of my pamphlet of 1966-67, this book consists of a set of essays and reviews written during a two-year period and bearing on various problems of interest.

As in any work of this kind, I am indebted to a host of authorities who have been my unwitting mentors. It is difficult to specify those who have had the greatest influence on me. There is of course Collinder, but there are also a number of others whom I regard with almost equal veneration and gratitude. My references to them are necessarily eclectic, and my interpretations of phenomena and my conclusions are of course my own, for which I accept full responsibility. If my work has any value, it is because I have gazed from the shoulders of giants.

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INDO-EUROPEAN PREHISTORY

2. INTRODUCTION

As stated in the Foreword, this book is a sequel to a pamphlet I wrote in 1966, and privately printed and distributed in early 1967, entitled "The Eurasiatic Pronouns and the Indo-Uralic Question"; since it is not generally available, an edited and shortened reprint of it is included in this book. At the time, I was concerned mainly with comparing the pronominal roots and affixes of Common IndoEuropean with those of similar function in Common Uralic and Common Altaic, counting the number of cases where the forms agreed in sound and function, and comparing the number of these agreements with that to be observed when any one of these three languages is compared with each of several other languages then believed to be unrelated, both among themselves and to each of the three families in question. Implicit was a recognition of the existence of an Altaic unity comprising Turkic, Mongol and ManchuTungus (and probably Korean and Japanese), which since the work of Nikolaus Poppe and Karl Menges can be regarded as about as well established as any other. Also implicit was an acceptance of a Uralic unity comprising FinnoUgric and Samoyedic, and also an IndoEuropean unity which included Anatolian. For convenience, I referred to these three linguistic groups as the "Eurasiatic" families, initially not implying that they are "genetically" related.

I made several statistical t-tests and found that the numbers of pronominal agreements among the three Eurasiatic families are statistically significant, and highly so. Accordingly, I concluded that this phenomenon of relatively numerous pronominal agreements must have had a unique historical cause. After considering various initially plausible explanations, I concluded that there were only two which now merited serious examination: 1. The possibility that the phenomenon was due to borrowing among the languages of the three Eurasiatic families, either when still undivided or later; and 2. The possibility that the phenomenon was a relic of an early prehistoric "genetic" relationship among the three undivided Eurasiatic languages. When we say that two or more languages are "genetically" related we mean that they separately evolved from a single earlier language ancestral to them all; of course, they need not have separated all at the same time.

In regard to the first possibility, I considered several cases where the speakers of different languages have been in intimate contact for centuries. Although transfers of lexical items have frequently been observed, and even occasional borrowing of grammatical devices, I found that the borrowing of pronominal forms, whether roots or affixes, while not completely unknown, is extremely rare, much too rare to account for any sig-

nificant statistical effect. I therefore concluded that the first possibility must be rejected.

This left only the second possibility as a credible explanation for the phenomenon in question -- provided, of course, that there were no considerations which militated against it. Among the Eurasiatic families, and among the "Nostratic" families generally, it is clear that IndoEuropean shows greatest similarity to Uralic so far as pronominal and morphological agreements are concerned, although in vocabulary it seems to be closer to AfroAsiatic. Consequently, and because of the inordinate interest which IndoEuropean holds for us whose roots are in Western Civilization, it is the connections with these two families whose possibilities have engaged the greatest attention. Various objections have been raised against the IndoUralic connection. For example, Collinder in 1934, in the introduction to his Indo-uralisches Sprachgut, cited objections which others had raised based on supposedly fundamental incompatibilities between Uralic and IndoEuropean in their phonetic and syntactic systems; other objections were based on a paucity of common terms for parts of the body and family relationships and for numerals, and on a paucity of common vocabulary generally, which contrasted with the relatively large number of similar pronominal and grammatical forms.

Collinder showed, however, that these objections have no substance, being based on academic prejudice resulting from the restricted experience of the majority of comparative linguists in their preoccupation with IndoEuropean. Since an IndoUralic connection would necessarily be more ancient than connections between coeval branches of IndoEuropean, or of Uralic, the relative paucity of evidence for the IndoUralic connection is entirely in order. In fact, a comparison of the discernible respective vocabularies shows quite clearly that the connection, if it existed, must have antedated the respective acculturations of the two undivided peoples to a Neolithic (food producing) way of life. Knowing this, and realizing that the wandering life of most hunter-gatherer groups is conducive to the spreading of tribes and languages over extensive regions (especially in a pre-Neolithic era), I came to the conclusion that there really is no rational basis for denying the IndoUralic "genetic" connection. This left me with no alternative but to accept the IndoUralic hypothesis as proved, the prevailing academic opinion notwithstanding. I still hold to this conclusion since I have seen no evidence or argument that would militate against it. The fact that this conclusion is not popular is of course irrelevant.

Collinder, however, has never publicly acknowledged the IndoUralic hypothesis as proved, despite the fact that his lifelong interest in the topic suggests that he has privately be-

lieved in it. In 1959, in the introduction to his Comparative Grammar of the Uralic Languages, he wrote "The Indo-Uralic hypothesis cannot be regarded as proved. The pronominal stems show a striking conformity, and there are some resemblances in the derivative suffixes and flexional endings too; but there are very few common words."

Up to the time I did my statistical work I agreed with Collinder, and when I completed the work I believed that I had supplied the necessary proof. I still believe that the work I did was valid and adequate as proof, but on recently rereading the Indo-uralisches Sprachgut I have come to the conclusion that the investigation I made was unnecessary, for I now believe that Collinder's information as then reported was itself more than adequate to stand as proof of the IndoUralic hypothesis, despite his disclaimers to the contrary -- or most certainly would have been if he had included an appropriate a posteriori statistical analysis. It should be noted that his objection based on the fact that "there are very few common words" is not a valid one; it is a relic of the academic prejudice which I mentioned above.

In fact, Collinder himself has more recently shown (in 1975, see item 1.5 in the bibliography) that if we were aware of only two IndoEuropean languages such as present-day spoken Greek and present-day spoken Swedish, we would find the number of common words to be only about 50 or 60, which is not much greater than the 30 or 40 that has been counted for Common Uralic in comparison with Common IndoEuropean. Collinder also referred to a similar study by Georg Morgenstierne who compared Irish with Afghan, finding even fewer common words than Collinder did. These results are especially surprising since the intra-IndoEuropean connections are considerably less ancient than any connection between Common IndoEuropean and Common Uralic. Thus, in at least these two instances, when widely separated present-day spoken IndoEuropean languages are compared on a pair-wise basis, without the advantage of material from extraneous and/or extinct languages, few common words are found. It should be noted that the comparison of Common IndoEuropean with Common Uralic is likewise done, necessarily, on a pair-wise basis since we are working with single (hypothetically reconstructed) languages; this being the case, the number of common words actually turns out to be surprisingly large considering the relative antiquity of this connection.

After all, how many common words are necessary in order to be regarded as adequate for "proof" of "genetic" relationship? Is it 1, 3, 10, 30, 100, 300, 1000 or 3000? If we select a number, and further require that the number be sufficient to enable scholars to establish a complete set of phonetic equations and to fully reconstruct the parent language in both its phonemic and morphologic aspects, then we have to concede that the exis-

tence of the IndoEuropean unity itself must be regarded as unproved. Anyone who doubts this should take note of the unsettled condition of laryngeal studies, for example. Special notice should be taken of the ferment that has recently arisen concerning the IndoEuropean consonantal structure in even its most fundamental aspects. Yet no one hesitates to accept the reality of the IndoEuropean unity despite the fact that the phonemic (and morphologic) reconstruction of Common IndoEuropean is still controversial even after 200 years of investigation, and despite the availability of such ancient languages as Vedic Sanskrit and Hittite and other extinct but historically preserved IndoEuropean languages. The difficulty with Common Uralic is even greater, for we have no records of any Uralic language as early as, say, Latin, or even as early as Old English.

The real reason why the IndoEuropean unity is accepted is that it meets (or almost meets) the fundamental requirement of reductio ad absurdum, which is the only available means of proving a positive proposition apart from direct observation of an event or situation. This technique of logic is particularly useful in cases where the issue is dichotomous, as in the present case: If we select any two "IndoEuropean" languages, then we can say that they both either separately evolved from a single earlier language, or they did not -- there are no other possibilities. If we support the negative and assert that they did not, then we must propose some other explanation to account for the observed resemblances. Some of the resemblances are undoubtedly due to borrowing, but not all of them, for these languages are remarkable more for divergence than for convergence; this explanation is rightly judged as insufficient. Or we could suggest that some Being suddenly created the Universe five minutes ago, together with all our memories and histories and all the linguistic and other phenomena that are now observable. I know of no way to refute this proposition except to invoke Ockham's Razor, for this suggestion becomes superfluous as soon as we find some other explanation.

However, the most fundamental potential objection against accepting the reality of the IndoEuropean unity is the suggestion that the observed resemblances may have developed independently. After all, how can anyone actually prove that such lexical resemblances as English "bear", Latin "fer-", Greek "pher-" and Sanskrit "bhar-", and such grammatical agreements as the plural in -s in English and many other IndoEuropean languages, are not due to separate and independent convergent developments? Let us remember that any direct observation of the underlying historical process (if any) is precluded in this case. Consequently, the existence of a historical connection among these phenomena can only be inferred.

2. Introduction

In case this question is considered trivial, perhaps someone will explain why it is that the use of pronominal endings in the nominative plural of masculine o-stem nouns in Greek, Latin, Lithuanian and Celtic is regarded as due to independent innovation, and why their non-use in other languages is regarded as primordial. My point is that such judgments are seldom supported by a formally stated process of reasoning which would show why all the available alternative suggestions should be rejected; thus they seldom meet the fundamental requirement of reductio ad absurdum.

In short, so far as I know, no one has yet offered a formal proof to show why the intra-IndoEuropean resemblances should not be regarded as due to independent developments. The mere fact that this proposition is universally rejected is irrelevant, for a popular vote has no significance where facts are concerned. Also, the assertion that the proposition is "unthinkable", as some may actually suggest, is not an argument. It isn't even true, for the proposition is indeed "thinkable".

What is lacking is a formal a posteriori statistical study to calculate the probability that the intra-IndoEuropean resemblances are due to independent developments. I strongly suspect that if such a study were actually performed it would show that the probability is extremely small, and in that case it would be legitimate to conclude that the proposition has been refuted beyond reasonable doubt. Since this would remove the last remaining argument against accepting the reality of the IndoEuropean unity, we would then be justified in concluding that the Indo-European languages are indeed "genetically" related -- assuming, of course, that there are no considerations that would militate against it. However, I am not aware that such a statistical study has ever been performed, and until it is, the proof of the existence of the IndoEuropean unity will remain incomplete since its acceptance, thus far, is essentially intuitive. For the purpose of this book, however, I will be charitable and assume that the unity is real.

Thus a belief in the IndoUralic unity is at least as well justified as a belief in the IndoEuropean unity itself, for the former has been buttressed by an a posteriori statistical study, consistently yielding high significance, while the latter has not (so far as I know); and I have shown that such a study is necessary in order to formally refute the proposition of independent convergence. The criterion of relatedness is not whether the languages in question exhibit a customarily accepted (hence culturally arbitrary) minimum number of common words together with a plausible set of phonetic correspondences, but whether they evolved from a single earlier language. Sometimes an a pos-

teriori statistical study can be an indispensable aid in determining this.

In fact, a "plausible set of phonetic correspondences" may actually give a misleading impression of the true prehistoric "genetic" connections of a language, especially in the earlier phases of its investigation. A good example of this is the case of Thai, which used to be regarded as a Sinitic language, with closest relationship to Chinese. More recent investigation divorces it from Sinitic and relates it more closely to Austro-Asiatic for much the same reason that English is more closely aligned with Germanic than with Romance; there are phonetic correspondences which, if considered superficially, suggest a "genetic" connection of English with Medieval French -- here I am ignoring the earlier IndoEuropean connection, of course. I have met educated Germans who actually tend to think of English as a Romance language.

While writing this book, I have become increasingly aware of the fact that the IndoUralic "genetic" connection, while definitely valid beyond reasonable doubt, cannot be adequately appreciated in isolation. Just as the connection between, say, Germanic and Slavic cannot be adequately treated without reference to other branches of IndoEuropean, neither can the connection between IndoEuropean and Uralic be adequately understood without also considering similarities between EskimoAleut and Uralic noted by Bergsland and Uhlenbeck, between Altaic and Dravidian noted by Menges, and between IndoEuropean and Afro-Asiatic noted by Bomhard -- to mention a few. The implication is that the existence of the Nostratic superfamily is real. It merits serious and persistent investigation.

My gradual realization of this fact is reflected in the choice of topics treated in these chapters, which are presented in the chronological order of their composition.

3. THE EURASIATIC PRONOUNS and THE INDO-URALIC QUESTION
(Edited Reprint)

***[(1982 NOTE: This is a shortened and edited reprint of my pamphlet of 1966-67. Because of the limitations of my printing arrangements, subscripts are shown as on-line suffixed letters, and italicized words are printed in bold-face, sometimes underlined.)]*

ABSTRACT

An a posteriori statistical study is made to determine whether the number of pronouns whose significant elements are similar in sound and function in the undivided IndoEuropean, Uralic and Altaic languages is sufficiently great to be regarded as "statistically significant". It is concluded that the numbers of such agreements are indeed significant and that most of the pronominal agreements are therefore not attributable to independent developments but instead were CAUSED by a single, UNIQUE, historical circumstance. The possibility that the noted agreements were caused by the borrowing of pronominal forms among the three undivided languages is examined and rejected.

The only remaining alternative is to accept the hypothesis that the three linguistic families are related (i.e., that they all separately evolved from a single earlier language ancestral to them all), for all other hypotheses are shown to yield consequences which are contrary to observation. Thus the IndoUralic hypothesis, among others, is confirmed.

*It is suggested that all three families are to be derived from V. Gordon Childe's Forest Culture, a Circumpolar Mesolithic culture which began in Northern Europe after the end of the most recent Ice Age and spread Eastward along the Circumpolar zone. The language of this culture may have been a continuum of closely related dialects which eventually broke up into a chain of related languages. One of these East of the Urals may have been ancestral to Common Altaic, another West of the Urals ancestral to Common Uralic. **[(1982 NOTE: My ideas on this topic have changed. I now tend to favor a more Southern origin for the common linguistic ancestor of the three families. Obviously, the suggestions in this paragraph are highly tentative.)]*

The currently most favored location for the homeland of the undivided IndoEuropeans is Southern Russia. This location is attractive to me because its proximity to the putative homeland of the undivided Uralic speakers adds to the credibility of the IndoUralic hypothesis. However, this is rejected for several reasons.

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**[(1982 NOTE: I still reject a South Russian homeland for the IndoEuropeans. There are about a half dozen reasons for this. This matter is discussed more fully herein.)]*

The undivided IndoEuropeans are to be identified with Childe's Danubian I (or Linear Pottery) Culture, the earliest Neolithic (food-producing) culture which has been archeologically observed in Central Europe and surrounding regions. This culture seems to have developed as a result of influences arriving from Anatolia via the Starchevo Culture in the Balkans. Of course, this does not imply that the IndoEuropean language and its speakers themselves originated in Anatolia, nor perhaps even in the Balkans (except the Northwestern part, adjacent to Hungary). Some of the historically known branches of post-IndoEuropeans are to be identified with various successors of the Linear Pottery Culture (Roessen, First Northern, Tripolye, Fatyanovo, etc.).

***[(1982 NOTE: Prior to 1966 I had shared the common stereotype of the undivided IndoEuropeans as an egregiously warlike, horse-riding group coming into Europe from somewhere outside, presumably South Russia or Central Asia like the Huns and Tatars. While writing my pamphlet, however, I began to realize that this stereotype has no evidence whatever to support it. This removed all objections to the theory of a Danubian origin, which on linguistic grounds is much to be preferred. This is still my opinion today.)]*

An explanation for the origin of gender in IndoEuropean is presented. This explanation requires an acceptance of the IndoUralic hypothesis as a necessary precondition.

***[(1982 NOTE: I am still completely satisfied that my explanation for the origin of gender in IndoEuropean is correct.)]*

3. The Eurasiatic Pronouns and the IndoUralic Question

(Reprint, Cont.)

INTRODUCTION

Man is a curious animal, in both meanings of the word. One of the things which we, as men, are curious about is language, for language is a cultural phenomenon whose study can often shed considerable light on the prehistoric relationships and movements of human groups. This is important because it enables us to gain some insight into the human process in diachronic as well as synchronic aspects, thus deepening our knowledge of who we are, how we came to be this way, and what we can do about our present problems.

Thanks to the work of many investigators during the last two centuries, we now know of the existence of a particular linguistic family, the IndoEuropean, which embraces the culture of many of us, and which today is the most widely spread of all. It was not always so. Much of this expansion is known to have occurred in historic times, and much of the earlier expansion in prehistoric times can be inferred circumstantially. These investigations have yielded an understanding of such concepts as linguistic family, linguistic change or evolution, linguistic diversification, linguistic spreading, linguistic mixing, and convergent linguistic evolution. In particular, some of the criteria needed to demonstrate relationships between languages have been elucidated, the main one being that the languages should have a number of words in common about which consistent phonetic equations can be written. This criterion has been successfully applied in identifying the main dialects of English (and other languages), the various languages within the Germanic group (and in other groups of like antiquity), and the major groups within IndoEuropean and other major linguistic families. When we say that certain languages are related we mean that they separately evolved from a single earlier language which was ancestral to them all.

The above criterion (which may be called Comparative Historical Phonology, or CHPh) has been particularly successful in cases where the "time depth" -- the amount of time which has elapsed since two specific languages separated -- is not too great. Working backward in time, however, we find that this method becomes less successful, for the number of identifiable common words becomes fewer and the accrued phonetic changes become greater, making it increasingly difficult to identify the few common words that still remain and to specify the phonetic relationships between them. Indeed, it appears that all identifiable evidence of relationship would probably disappear after a *mere 20,000 years, which is a disappointingly thin veneer on the prehistoric background of human speech. It is a tragic fact that our linguistic comparisons will never enable us to elucidate more than the most recent developments of prehistory.*

Thus CHPh has been highly successful in clarifying the linguistic relationships within such families as IndoEuropean, Uralic, Semitic, Athapaskan, Algonkin, etc., where the time depth is not much more than some 5,000 years, but it breaks down completely where the time depth is 20,000 years or more.

What about cases in the intermediate range, those cases where the time depth is something between 5,000 years and 20,000 years? This is a matter of great interest because if we try to demonstrate a relationship between some of the above families (for example), we must use comparative techniques appropriate for time depths in this range. This is important because it is reasonable to assume that the same process of linguistic diversification and spreading, known to have occurred in historic and late prehistoric times, must have occurred also in earlier prehistoric times, for man's essentially human condition is far older than a mere 20,000 years.

As stated above, CHPh loses much of its usefulness when applied to cases where the time depth is in the intermediate range, for the results are usually inconclusive or illusory at best. Such is the case in regard to well known attempts to relate IndoEuropean to Semitic or Uralic, for example. Collinder in 1960, in the Introduction to his Comparative Grammar of the Uralic Languages, wrote "The Indo-Uralic hypothesis cannot be regarded as proved. The pronominal stems show a striking conformity, and there are some resemblances in the derivative suffixes and flexional endings too; but there are very few common words."

What does "striking conformity" mean? To me, an electronic communications engineer, the expression has no meaning except possibly in statistical (quantitative) terms. It suggests that there may be a "statistically significant" number of cases where a pronoun in one family has the same, or nearly the same, phonetic form as the corresponding pronoun in the other family. Collinder's remark asserts at once the possibility of this situation and the inapplicability of CHPh to the general vocabulary.

It appears, then, that Collinder has presented us a new criterion, or method, which may prove useful in a particular case where CHPh fails -- the case involving the IndoUralic hypothesis. My purpose here is to develop and refine this method and to apply it in an attempt to decide the question as to whether or not the IndoUralic hypothesis ought to be accepted beyond reasonable doubt. The IndoUralic hypothesis states that Common IndoEuropean and Common Uralic separately evolved from a single earlier language. Common IndoEuropean and Common Uralic are the *(partially) reconstructed languages from which the historically known IndoEuropean and Uralic languages respectively evolved.*

A DESCRIPTION OF THE METHOD

Let us call the new method "Comparative Pronominology". The process may be divided into two parts. In the first part we make a statistical calculation designed to reveal, beyond reasonable doubt, whether the number of cases where the pronouns of Common IndoEuropean are similar in form and function to those of Common Uralic, are sufficiently numerous to be regarded as "statistically significant". If the answer is "No", then there would be no need to do Part Two, for we would be already compelled to regard Collinder's intuitive impression of "striking conformity" as illusory. The IndoUralic hypothesis would remain unproved.

On the other hand, if the answer to Part One is "Yes", then we must proceed to do Part Two, for we would be compelled to regard the "striking conformity" as genuine and therefore CAUSED by some UNIQUE historical circumstance. Part Two consists of a process of reasoning intended to identify the historical cause.

PART ONE: THE STATISTICAL CALCULATION
(a posteriori)

In the course of the independent development of pronominal systems in any two mutually quasi-alien languages we can expect that a few of their pronominal forms will accidentally be similar in form and function. This is because all known languages have only a finite (and rather small) number of phonemes, and the number of separate pronominal functions in a given language is sufficient to make it seem probable that any two languages, however alien their origin and development, will present at least a few cases where a given pronominal function will be expressed by forms which have similar sounds in the two languages.

How similar? This is a controversial point. Any a posteriori probability calculation necessarily involves a process of counting, and we cannot do this without first identifying, and discriminating among, the objects to be counted, and assigning each of them to one of two or more categories. This process of identification and assignment often involves some degree of arbitrariness -- hence the controversy. In this investigation the objects we are counting are the CASES where two different languages use a similar phonetic form to express a given pronominal function.

Again, how similar? Some degree of arbitrariness seems unavoidable here, for it is known that not all languages have the same phonemes and allophones, and most pairs of languages therefore will not be fully comparable if we insist on complete identity of significant phonetic elements in order to establish a "case" as an object to be counted.

A further "looseness" arises from the fact that not all languages express the same pronominal functions.

Still another uncertainty is involved in attempting to determine just which phonetic elements in a given pronominal form are "significant" in expressing a particular pronominal function.

The importance of this last problem is seen when we consider the -n- in Finnish "minae" and "sinae" ("I" and "thou"), for example. Here the -n- seems to be a vestige of an ancient UralAltaic demonstrative element used to designate pronouns in general, without isolating such functions as "first person singular" and "second person singular". Accordingly, if Finnish were one of two languages being compared, the -n- in "minae" and "sinae" ought not be considered "significant"; it should not be allowed to contribute toward qualifying either of these pronouns as participants in a "case".

On the other hand, this same -n- appears to be the historical origin of an -n or -ng used to denote the second person (singular and plural) in ObUgric and Turkic languages. If one of these languages were involved in a comparison the -n or -ng should accordingly be taken as a significant phonetic element for the second person singular and the second person plural for that specific language. These morphemic developments seem to be secondary in these languages, post-dating the Common Uralic and Common Altaic periods.

CONCLUSIONS OF THE STATISTICAL STUDY

The details of the statistical work are shown in the two chapters comprising the Statistical Appendix, entitled STATISTICAL DATA and ANALYSIS OF THE DATA. It will be noted that I have utilized pronominal affixes as well as independent pronouns. Also, the significant phonetic elements were chosen solely on the basis of function in the particular language concerned, without regard to historical origin; this principle is required by the assumption of independence which is implied by the use of the null hypothesis.

As just mentioned, the study begins with a "null hypothesis", tentatively assuming that the pronominal similarities among the three Eurasiatic families (IndoEuropean, Uralic and Altaic) developed independently of one another.

3. The Eurasiatic Pronouns and the IndoUralic Question

The study concludes that the pronominal agreements among the three families (especially between IndoEuropean and Uralic) are definitely too numerous to be reasonably regarded as independently evolved, thus refuting the null hypothesis.

This means, of course, that Collinder was right; the pronominal stems in IndoEuropean and Uralic do indeed show a "striking conformity". More specifically, it means that the pronominal conformities among the three Eurasiatic families (and especially between IndoEuropean and Uralic) were CAUSED by some UNIQUE historical circumstance.

PART TWO: DISCOVERING THE HISTORICAL CAUSE
(Edited Reprint, Cont.)

Let us present the following four hypotheses for consideration:

1. That the pronominal conformities among the three Eurasiatic families (and specifically between IndoEuropean and Uralic) are due to independent developments.

2. That the noted conformities are due to some genetically inherent quality in human minds which would make it probable that just those phonetic elements would tend to be associated with just those pronominal functions, at least where people in the Eurasiatic geographical area are concerned.

3. That the noted conformities are due to borrowing of pronominal elements among the undivided languages of the three Eurasiatic families.

4. That the noted conformities are due to the separate evolution of Common IndoEuropean, Common Uralic and Common Altaic from a single still earlier language ancestral to them all; that is, that the three Eurasiatic families are related.

I am not aware of any other hypotheses that may reasonably be considered as requiring investigation.

The first hypothesis has already been eliminated in Part One.

The elimination of the first hypothesis also automatically eliminates the second, for the alternative would be to assert that the speakers of non-Eurasiatic languages are somehow non-human when regarded from the point of view of speakers of Eurasiatic languages. The second hypothesis is further refuted by the frequently observed fact that people have no difficulty in learning ANY foreign language, including its pronominal forms, "like a native" if they are associated with it at an early enough age.

PRONOMINAL BORROWING

The third hypothesis states that the noted pronominal conformities among the three Eurasiatic families are due to borrowing of pronominal forms among the three undivided languages. How credible is this hypothesis?

Intuitively, the hypothesis seems entirely plausible. The unbound pronouns in a given language are, after all, a part of its vocabulary, and it is well known that the vocabulary of a language is particularly likely to have been obtained, in part, by borrowing from other languages.

However, there also is reason to suspect that not all parts of the vocabulary are equally likely to be borrowed. It may be that the rate of borrowing of pronouns is different from that of other parts of the vocabulary.

To determine whether this is so, let us consider to what degree pronoun borrowing has occurred in cases of contacts between quasi-alien languages of whose histories we have some direct knowledge. For the moment, let us confine our attention to contacts dating from historic or late prehistoric times.

For example, Hungarian has been in intimate contact with Slavic and Germanic languages for a thousand years and has borrowed many words from them, but all the Hungarian pronouns, both roots and affixes, appear to be derivable from FinnoUgric.

Likewise, Finnish has borrowed many words from Baltic and Germanic languages during the last 2,000 years, but are any of the Finnish pronominal forms derivable from other than FinnoUgric? I believe not. We are of course reminded of Finnish "haen", which appears as if it might have been borrowed from Swedish "han", but Collinder in his Fenno-Ugric Vocabulary derives the Finnish word from FinnoUgric.

Again, Welsh has been strongly influenced by English for more than a thousand years, but were any of the pronominal forms borrowed? I doubt it.

Also, Rumanian has been separated from the other Romance languages, and surrounded and influenced by other Balkan languages for more than a thousand years, but I do not believe that any of its pronominal forms are derivable from non-Latin sources.

Again, Sturtevant and Hahn, in their Comparative Grammar of the Hittite Language, have shown that Hittite was strongly influenced by Akkadian and other nearby languages, yet all the Hittite pronominal forms appear to be derivable from Indo-European.

Likewise, Basque has been surrounded by various Indo-European languages for 2,000 years, yet all its pronominal forms appear to be indigenous.

Instances such as these lead me to the conclusion that occurrences of pronoun borrowing are extremely rare. It is not completely unknown, however. For example, our dictionaries attribute the English forms "they" and "their" to Old Norse. The circumstances, though, are peculiar. Jespersen, in his delightful book on the Growth and Structure of the English Language, says that the Norsemen came to the North of England in great numbers. They fought against the English as relatives and were absorbed and settled down as relatives. The two peoples were alike in language and culture (except for religion) and it seems likely that they could understand each other without too much difficulty. The two languages merged with each other in an extremely intimate way, a way which suggests the fusion of divergent but obviously related dialects, not of fully alien languages. So intimate was the fusion that numerous Norse loan words occur in the very heart of the Germanic core of English speech, even in the pronouns. The borrowing of "they" and "their" was probably aided by the analogy of "the", "that" and "this" and by the fact that the native 3rd plural forms were easy to confuse with the singular forms because of the phonetic merging which had tended to obscure the discrimination between singular and plural.

I regard as spurious the folk etymology which attributes "one" (as in "one says") to French "on". And even if this "borrowing" were genuine, it still is not a part of ordinary English or American speech.

The only other example of pronominal borrowing that I know of (and even it is doubtful) occurs in Brahui, a Dravidian language isolated in Baluchistan. It has long been surrounded by IndoIranian languages and has been much influenced by them, with much borrowing of vocabulary under conditions of intense bilingualism. In his Grammatical Structure of the Dravidian Languages, Bloch lists the personal subjective terminations of verbs in Brahui as follows:

	sing	plur
1	-iv	-in
2	-is	-ire
3	-e	-ir

Bloch continues (as translated by Harshe): "The first two persons of the singular present the most serious problem: 1) -iv (pronoun i), 2) -is (pronoun ni) are only understood if it is admitted that they are of Aryan origin, and considering the -s of the 2nd person, of Indian origin; on the other hand, among *the Indo-Aryan languages of the West, only the European Gypsy furnishes, at the same time, the terminations in question (kamav "I love", kames "thou lovest"; see J. Bloch, Indo-Aryan, JRAS., 1946, p. 199 ff)."*

I conclude from this discussion that genuine instances of pronominal borrowing are extremely rare, that they occur only under peculiar circumstances, that these circumstances always involve either the intimate fusion of closely related languages or the overwhelming dominance of one quasi-alien language by another with which it has long been in intimate contact, and that both of these circumstances always entail the extensive borrowing of lexical items in the general vocabulary of at least one of the languages, resulting in a large fund of common vocabulary. It follows that if two languages have several pronominal elements in common, and if they do not have a large common vocabulary, then the common pronominal features are not due to borrowing.

In any case, pronominal borrowing has never been known to occur to a degree sufficient to account for such uniformly and emphatically significant statistical phenomena as we have noted in the Statistical Appendix. THIS IS THE IMPORTANT POINT. Thus pronominal forms are indeed a privileged part of the vocabulary (and morphology).

*Let us now apply this conclusion to the IndoUralic question. It is desired to determine whether the large fund of pronominal forms held in common by Common IndoEuropean and Common Uralic is due to borrowing from one language by the other. If it is, then we must expect to find a large number of (nonpronominal) words, dating from the period of Common IndoEuropean and Common Uralic, which are similar in sound and meaning in the two languages. This of course excludes the many loan words in individual Uralic languages which date from the Common FinnoUgric period and later. **[(1982 NOTE: Collinder has repeatedly made the point that there was no borrowing between the two undivided languages. This shows quite clearly that the two undivided peoples lived in regions which were sufficiently separate to prevent any significant influence between them.)]*

However, Collinder in his Indo-uralisches Sprachgut lists only some 30 or 40 items of common vocabulary dating from the period of Common IndoEuropean and Common Uralic, which seems to have been around the fifth or sixth millennium BC. As he remarks in his Comparative Grammar, "there are very few common words". Therefore it is utterly unreasonable to regard the numerous pronominal agreements between the two languages as due to borrowing, else the number of common words would surely be many times greater than it actually appears to be.

Further, there are additional reasons to believe that there was little contact between the speakers of the two languages for a period of several thousand years prior to the era of Common IndoEuropean and Common Uralic. This becomes obvious when we consider the profound differences between the two languages. If there had been much contact between the two peoples we should expect considerable convergence of linguistic development, as in the recent convergence of Lappish, Finnish and Hungarian to Neo-IndoEuropean patterns of sentence structure, and also in the phonetic systems. Instead, the phonetic system of Common IndoEuropean is sharply different from that of Common Uralic in its remarkable splitting of stops in unvoiced, voiced and aspirate series, in addition to its fairly frequent use of initial compound consonants. These features, added to the use of ablaut, reduplication, gender, the declension of adjectives in agreement with associated nouns, and the lack of vowel harmony, show that the development of the two languages was strongly divergent, not convergent. This means that the sustained intimacy of contact, needed for any notable borrowing of pronominal forms, was lacking.

Similarly, the same arguments (except for the last paragraph) can be applied to show that the pronominal roots and affixes common to Uralic and Altaic were likewise not due to borrowing.

Thus the third hypothesis is refuted.

CONCLUSIONS

In the foregoing discussion I have eliminated all except the fourth and last hypothesis, which states that the noted conformities are due to the separate evolution of Common Indo-European, Common Uralic and Common Altaic from a single earlier language ancestral to them all (Of course, they need not have separated all at the same time.) Is this hypothesis plausible or implausible?

There is certainly nothing to make it implausible. We know of such widely spread linguistic families as the IndoEuropean, Turkic, Athapaskan, Bantu, MalayPolynesian and HamitoSemitic (or AfroAsiatic), all of which spread in fairly recent times; and there is certainly nothing to negate the hypothesis that the same sort of spreading occurred also in earlier times, such as the Upper Paleolithic and Mesolithic periods. Certainly, the wandering life of hunters and food gatherers would be conducive to the spreading of languages over extensive regions.

**[(1982 NOTE: This is undoubtedly the reason why mankind is still a single species; the spreading and mingling of tribes *and genes, through the ages, has been so extensive that there has been no opportunity for genetic differentiation to progress to the point where a separate species might be permanently established. Man is peripatetic and, eventually, all-embracing.)]*

Since there is nothing to negate the fourth hypothesis, and since all the other hypotheses have been eliminated, I have no choice but to accept the fourth hypothesis. This is an application of the logic principle called reductio ad absurdum, by which all hypotheses except one are shown to yield consequences which are contrary to observation.

I therefore conclude that the three Eurasiatic families are related; in particular, the IndoUralic hypothesis is confirmed. This is the only conclusion whose consequences are not contradicted by the observable facts.

INDO-EUROPEAN PREHISTORY

THE INDO-URAL-ALTAIC HOMELAND
(Edited Reprint, cont.)

In the previous chapter it was concluded that Common IndoEuropean, Common Uralic and Common Altaic separately evolved from a single earlier language, for the available pronominal evidence does not allow any other conclusion to be drawn. Scholar's next task is to try to gain some insight into the nature of the language, culture and geographical location of the population which was linguistically ancestral to all three families. This present attempt will be crude and superficial, partly because it is peripheral to the main intent of this pamphlet, and partly because not much work has yet been done in this field. Much of what I will say on this topic will be premature and speculative. Obviously, many of my conclusions will be tentative.

A preliminary step would be to attempt the same task separately for Common IndoEuropean, Common Uralic and Common Altaic. Unfortunately, many questions involved in the resolution of the three subtasks are still disputed by specialists in comparative linguistics and archeology.

The least controversy seems to be involved in suggesting a location for the undivided Uralic people. The consensus seems to be that they occupied Central and Eastern Russia, the general area which includes Moscow, Kazan and Perm. I see no reason to dispute this.

The case for Altaic is less satisfactory. Archeological and linguistic evidence are both too scanty and too ambiguous to be of much help. Offhand, I would guess that it might have been anywhere between Lake Balkash and Lake Baikal -- perhaps the Altai Mountains or the plains immediately Northwest of them.

The currently most favored location for IndoEuropean is Southern Russia -- North of the Black Sea, the Caucasus and the Caspian Sea. Among linguists, this is suggested by Collinder (see 1.3, p 37) and Crossland (19.1, pp 16 - 46); among archeologists, it is suggested by Gimbutas (20.1, pp 815 - 836) and Piggott (21.1, p 82), who follows Gimbutas and Crossland. In my own reading of Crossland's remarks (19.1, p 37) I get the impression that his views are considerably more equivocal than Piggott's reference to him seems to suggest. Based on interdialectal relationships, Crossland seems to favor a homeland placed originally in the basin of the Danube (Southwest and South of the Carpathians), perhaps extending Eastward to the North of the

Black Sea. In this, his views are fairly close to my own, as will be shown below. On the other hand, he also seems to favor a relatively Eastern region (South Russia, presumably) on the basis that this would provide a region close enough to the FinnoUgric homeland to account for the similarities between IndoEuropean and FinnoUgric. But I will show that it does not follow that South Russia was the IndoEuropean homeland.

If the first two views are correct, it would seem reasonable to suggest that the Common Uralic- and Common Altaic-speaking communities, at least, separately formed out of the same Mesolithic culture, the former West of the Urals and the latter East of the Urals. This may have been Gordon Childe's Maglemose or Forest Culture, for both the Uralic and the Altaic peoples are of Circumpolar origin, and the Forest culture is the only Mesolithic Circumpolar culture yet discerned here. In 15.1, pp 9 - 12, Childe briefly describes this culture as a highly successful adaptation of a savage Upper Paleolithic economy to post-glacial conditions in the Western half of the Eurasian Circumpolar zone. Apparently it began in pre-Boreal times in Northwest or Northern Europe (radio-carbon date: 7500 BC at Star Carr, Yorkshire) and spread Eastward at least as far as Finnland during the Boreal period (perhaps 6000 BC to 5000 BC), and at least as far as the Urals by 4000 BC. In fact, the lakes and pine woods environment which then characterized the Forest habitat extended not only to the Urals but also across Northern Siberia and even Northern North America. There has been a great deal of cultural uniformity all along this zone, for there was very little impediment to East-West migrations throughout this tricontinental Circumpolar belt in Boreal times, and East of the Baltic this ease of communication has persisted to a much later period.

Such, at least, was the case so far as the natural environment was concerned. There were no significant mountain barriers (the Northern Urals being insignificant), and even the Bering Strait was connected part of the time. And even when it was not connected it could still be crossed by the sort of boats which the Circumpolar Mesolithic peoples had, moving from island to island. In particular, the lakes and rivers were no impediment, but were actually a convenience. Even in the winter they could be crossed by sleds.

But what about the human pressures? Why was not the Eastward spread of the Forest people countered by a Westward spread of peoples from East of the Urals? To a minor extent, it was. There is skeletal evidence of a few Mongoloid individuals appearing West of the Urals among the Lapponoid and Europoid peo-

ples (15.1, p 209). On the whole, though, the major drift of population along the Eurasian part of the Circumpolar zone was definitely Eastward. I think this was probably due to the fact that the way to America lay open, presenting an empty continent which continually drained off the excess populations of Asia at its Northeast corner. Thus as groups of hunter-fishers moved Eastward across the Straits they left vacant territories behind them in Asia, which then were quickly reoccupied by Mongoloid and Causasoid groups from farther South and West (respectively). These in turn vacated lands still farther South and West, and so on. Thus Siberia continually invited immigration of Mongoloids from East Asia and of Caucasoids from the Western half of the Eurasian Circumpolar zone.

In Europe the situation was quite different. There was a Northward and Northwestward immigration of Caucasoid savages and (later) Neolithic farmers into Europe from Africa and Southwest Asia. This may have been due partly to the increasing dessication in those regions following a Northward shift of rain-bearing winds at the end of the Ice Age, and partly to the land-hunger of inefficient Neolithic farmers. The resulting population pressure in Europe had no ready way of relief except by an Eastward migration along the Circumpolar zone, or else Northeastward through Central Asia, but this must have been partially preempted by populations expanding Northeastward from the Near East. The result was the observed long-continued Eastward spread of Childe's Forest people.

This question might be raised: If the relatively high population pressure in Europe, together with the relatively low pressure in Siberia, was causing a general Eastward spread of the Forest culture, why was not this culture displaced from its Northwest European position? It was, to a minor degree, but only in Britain. Elsewhere the Forest tradition and techniques were either maintained intact (East of the Baltic) or else aggressive adjustments were made to take advantage of new economic opportunities while maintaining the local geographical position and basic Forest tradition and techniques. The prime example of the latter case was the Erteboelle culture in Denmark. I think the answer to the above question is that the Forest people were unusually aggressive and adaptable in exploiting the economic opportunities of the North, and the same general attitude of aggressiveness was probably maintained in the face of human challenges. Human aggressors were either repelled or absorbed.

In view of all this, it is easy to see how both the Uralic and the Altaic peoples could have developed out of the same Mesolithic culture, deriving eventually from the Maglemose or

Forest culture of Northern Europe. This includes the assumption that this culture was essentially unilingual, presumably as a continuum of contiguous closely related dialects which eventually broke up into a chain of related languages. One of these languages eventually was ancestral to Common Uralic and another to Common Altaic.

In case anyone raise an objection to the above suggestion on the basis of race -- i.e., that the derivation of Uralic and Altaic peoples from Northern Europe would require that they be Caucasoids -- the observation can be made that the Uralic peoples, at least, are overwhelmingly Caucasoid (either Lapponoid or Europoid); among them, only the Northeastern Samoyeds (Tavgi or Nganasan) seem predominantly Mongoloid. Among the Altaic peoples the Mongoloid element is obvious, being strongest among the Eastern members (Mongol and Manchu-Tungus-Korean-Japanese), the Western members (Turkic and Chuvash) having a greater Caucasoid element -- this seems to suggest an early mixing of Caucasoid and Mongoloid populations wherein a predominantly Mongoloid element presumably adopted the language of neighboring Caucasoids speaking an Eastern version of Eurasiatic.

Let us turn now to the IndoEuropeans. If Collinder, Gimbutas and Piggott are correct in locating the Common IndoEuropean homeland in South Russia, then we can readily imagine how the undivided IndoEuropeans could have been of Forest origin along with the Uralic and Altaic peoples. They would have been geographically contiguous to the homeland of the undivided Uralians, or nearly so, and racially they would have been fully as Caucasoid if not more so. So it is easy to see how a peripheral group of Forest people in South-Central or South Russia could have become acculturated to Neolithic and Eneolithic influences radiating Northward from the Near East via the Caucasus. We could assume that they had adopted an Eneolithic economy, including solid-wheeled vehicles drawn by oxen, by the middle of the third millennium BC, simultaneously evolving their local Forest dialect into what we call the undivided IndoEuropean language -- the geographical uniformity of their habitat, together with their mobility, would surely favor uniformity of language. Then, with Gimbutas, we could say that they invaded the Balkans and Central and Northern Europe around 2300 BC, thus imparting the IndoEuropean language to those regions for the first time. Such a theory is attractive to me because it would indeed be consistent with the IndoUralic hypothesis, which I accept.

The only thing wrong with the above picture is that I don't believe it. I don't believe it fits the archeological and (especially) linguistic facts, despite the attraction which this sce-

nario used to have for me, and which it still seems to have for many scholars.

**[(1982 NOTE: At this point I will discontinue the reprint of this section. There are several reasons for this. The major one is that I no longer regard the Forest Culture of the Circumpolar region as the most likely vehicle for the Eurasiatic Language and its prehistoric expansion. For one thing that assumption fails to account for the increasingly apparent connection of the Eurasiatic families, particularly IndoEuropean, with AfroAsiatic and other Southern groups. Consequently, for this and other reasons which have since become apparent, I now favor Central Asia (at about the time of the end of the most recent Ice Age) as the most likely setting for Eurasiatic.

Nevertheless, some of the argumentation I presented is still valid and will be incorporated into parts of this book as appropriate. -- JCKerns)]

THE ORIGIN OF GENDER IN INDOEUROPEAN
(Edited Reprint, cont.)

Acceptance of the IndoUralic hypothesis enables us to explain the origin of gender in IndoEuropean.

In the first place, it is clear that gender was a secondary and superficial development in early Common IndoEuropean. It was entirely absent in the inflections of the finite verb, for example, and even in nouns and adjectives the dichotomy between feminine and masculine-neuter was maintained clearly only in aa- and o-stems, and even here there was confusion in Latin and Greek. Further, the dichotomy between neuter and masculine (or masculine-feminine) originally was formally maintained only in the nominative and accusative cases. In fact, the Anatolian languages give direct historical evidence that the feminine in -aa in IndoEuropean-proper was a particularly late development which must have occurred after the separation of pre-Anatolian. These traits are in striking contrast to the characteristics of gender in HamitoSemitic languages, where the dichotomy between masculine and feminine (more fundamentally, between large or important versus small or unimportant) has pervaded all the major grammatical categories, including finite verbs, since the very earliest period that can be elucidated by the comparative method.

*Of course, the feminine in -aa was not the only feminine suffix which developed in early post-IndoEuropean. Earlier there was a suffix in -ser or -sor which had considerable use in designating females in Anatolian languages. However, in the rest of IndoEuropean this suffix never attained more than an abortive development because of competition due to the general adoption of the feminine in -aa as an adaptation of a neuter collective in -eH or -oH (here H represents an unspecified laryngeal); vestiges may be found in *swesor (sister, "she who is one's own") and in Latin "uxor" (wife, "she who is at the house"). In principle, however, all feminine suffixes were secondary in origin, clearly arising as adaptations of the basic process of suffixation inherited from Common IndoUralAltaic and well developed in all three of its major branches.*

However, the dichotomy between neuter and common is a fundamental feature of the very earliest form of IndoEuropean that can be reached by the comparative method. Apparently its development was a distinctly less superficial phenomenon than in the case of the feminine suffixes.

This being the case, it is curious that the neuter-versus-common dichotomy never affected the finite verb as it did the quasi-nominal parts of speech (nouns, pronouns, gerunds, adjectives and participles), especially since the finite verb itself seems to have been of gerund or participial (hence quasi-nominal) origin, as also in Uralic and Altaic. This contrasts strangely with the situation in HamitoSemitic, where the masculine-versus-feminine dichotomy is maintained about as thoroughly in the finite verb as in other grammatical categories.

Another remarkable feature was the fact that common nouns (i.e., nouns of common gender) always had the accusative singular in -m, whereas neuter nouns never did (except in o-stems, but that was late and had a different historical origin); also, common nouns had the nominative singular in -s (except in aa-stems, but those likewise were late; and except in r-stems and n-stems, but those must have been too early).

Further, the accusative singular of neuter nouns was always identical to the nominative, being always endingless except in o-stems.

These anomalies of the IndoEuropean gender system (mixture of systems, rather) are seen to be highly idiosyncratic when comparison is made with the systems in other linguistic families which have gender or noun classes (HamitoSemitic, Dravidian, Bantu). It is reasonable to suspect that the historical origin of the system was equally peculiar.

*The cause of these anomalies can be seen when we take into account Prof. Bo Wickman's conclusions in 18.1. He concludes that the Common Uralic accusative singular in -m was seldom used except when the noun was definite; when the noun was indefinite the accusative singular was usually identical to the endingless nominative. That this usage was inherited from Western dialects of Common IndoUralAltaic is particularly credible when we recognize that it is characteristic of Altaic languages to this day (though with endings derived from *-ig in Mongol and Turkic).*

As implied earlier, the IndoEuropean accusative in -m was attached only to common nouns, never to neuter nouns (except o-stems, of course). As Burrow shows in 2.1, the earliest discernible basis of differentiation between common and neuter was the dichotomy between agent nouns and action nouns. Originally these nouns existed in pairs (one an agent noun, the other an action noun), both members derived from the same root and nearly identical in form (often disyllabic), but with the former accented on the suffix and the latter on the root. Semantically, the agent noun was derived from the corresponding *action noun by epithetically representing any person or thing which was somehow associated with that action. Agent nouns were common and had the accusative singular in -m (like definite nouns in Common Uralic), whereas action nouns were neuter and had the accusative singular endingless like the nominative (like indefinite nouns in Common Uralic). Clearly, the same noun in Uralic could be either definite or indefinite, depending on application. Similarly, in IndoEuropean the same noun could be either an agent noun or an action noun, depending on application.*

This explains the whole thing. As I have said, there is good reason to believe that the Uralic system was inherited from Western dialects of Common IndoUralAltaic; hence it is reasonable to suspect that the same usage was characteristic of IndoEuropean also in the earliest stages of its separate existence. Action nouns were indefinite and had the accusative singular identical to the endingless nominative -- precisely as in Common Uralic; on the other hand, the same noun could be used as an agent, whereupon it immediately became definite and naturally took the -m accusative -- precisely as in Common Uralic.

This explains the peculiar character of gender in IndoEuropean. Unlike the gender systems in other families, the system in IndoEuropean was not originally based on any such antithesis as large-small, important-unimportant, male-female, or even animate-inanimate; instead, it developed very naturally out of the dichotomy between definite and indefinite inherited from Common IndoUralAltaic. Since this dichotomy affected only semantically nominal parts of speech, it is easy to see why gender never intruded into the IndoEuropean finite verb, despite its quasi-nominal formal origin. Also, since the dichotomy formally involved only the presence or absence of the -m accusative, it is easy to see why the same mark was originally the sole means of formally differentiating between common and neuter in IndoEuropean (the nominative singular in -s was clearly secondary, as shown by early common r-stems and n-stems without -s). Further, we can easily see why the accusative singular of neuters in IndoEuropean was identical to the nominative, for this was typical of indefinite nouns in Common IndoUralAltaic.

As stated above, the earliest discernible basis of differentiation between common and neuter in IndoEuropean was the dichotomy between agent nouns and action nouns, the discrimination being based on the dichotomy between definite and indefinite inherited from Common IndoUralAltaic. These nouns existed in pairs, both members derived from the same root and formally similar in that they generally had the same suffix also. Yet despite the formal similarity, the semantic difference was acute. We know this is true, not only because agent nouns took the -m accusative (being thought of as definite), but also because they were marked by a shift in accent. In course of *time, however, one member or the other tended to fall out of use, being replaced by some other word of unrelated origin. Eventually, after this lexical erosion and regeneration had gone on long enough, most of the action nouns and agent nouns no longer existed as phonetically similar pairs. Thus the formal basis for the dichotomy between definite and indefinite no longer existed, for the most part, so that dichotomy was eventually given up. It is at this point that gender, as we know it, really began in IndoEuropean, for the surviving member of a given pair was now usually isolated and, in effect, permanently assigned to one of two categories. Subsequent classifications of nouns tended to be made on a purely formal basis by several different phonetic criteria at different times; this is why the various manifestations of gender in the historically known IndoEuropean languages are so illogically organized.*

***[(1982 NOTE: I still am thoroughly convinced that the above explanation for the origin of gender in IndoEuropean is correct. I present a standing challenge to anyone who thinks it can be refuted. -- JCKerns)]*

STATISTICAL APPENDIX I
(Reprint cont.)

An A Posteriori Calculation of Pronominal Agreements

STATISTICAL DATA

I shall start with the following null hypothesis: That the development of the pronominal systems in Common IndoEuropean, Common Uralic and Common Altaic occurred independently, and that any observed similarities among them are therefore due to chance. Accordingly, it is fully as valid to use pronominal forms of the three undivided languages as to compare (say) New English with New Finnish or New Mongol. On the other hand, if the null hypothesis proves to be wrong, the use of the earlier forms will be a great help to us in making the decision more reliable and in identifying the historical cause of the similarities. This is in accord with a well known practice in comparative linguistics, wherein we always try to use the earliest available forms whenever they are applicable in a given comparison.

For convenience, the above three linguistic families will be referred to as the Eurasiatic group.

For the purpose of this investigation I shall group the major phonetic elements into categories as follows:

Category	***Phonetic Elements***
A	*Dental Consonants, voiced and unvoiced, stops, spirants and aspirates; also ch ("church"), j ("judge"), sh ("sheet") and zh ("pleasure").*
B	*s, z, ts, dz.*
C	*Labial and Labio-Dental Consonants, voiced and unvoiced, stops, spirants and aspirates.*
D	*Velar, Laryngeal and Glottal Consonants, voiced and unvoiced, stops, spirants and aspirates.*
E	*m.*
F	*n and ng (velar n).*
G	*w.*
H	*y.*
I	*Front Vowels.*

J Back Vowels.

K r, l.

The reader will note that the above grouping is based mainly on similarity of locus of articulation. It embodies, of course, considerable arbitrariness. **[1982 NOTE: Other investigators -- and I hope there will be many -- would probably define the categories somewhat differently. The main caveat is that, regardless of what system of categorization may be chosen, the system must be rigidly maintained throughout the investigation.]

Using these eleven phonetic categories as a standard, I shall list what I believe to be the significant phonetic elements in the pronominal systems of the three undivided Eurasiatic languages, together with the categories appropriate for those elements.

Then I shall count the number of different category agreements (pertaining to a given pronominal function) to be found in comparing Common IndoEuropean versus Common Uralic, Common IndoEuropean versus Common Altaic, and Common Uralic versus Common Altaic; then I shall add to get the total for all pronominal functions involved in each of the three two-by-two comparisons. The demonstrative and 3rd person functions (both singular and plural) will be considered as a single function, for many languages use the former to represent the latter.

Next, I shall compare each of the three Eurasiatic languages with each of several other languages (each believed to be alien to them and to each other), precisely as in the above comparisons among the three Eurasiatic languages. The number of category agreements observed in each comparison will be recorded and counted separately for each of the three Eurasiatic languages.

Further details will become clear as we proceed.

THE EURASIATIC PRONOMINAL DATA

In Burrow and Sturtevant it appears that the pronominal forms of Common IndoEuropean had the following elements:

1st sing: -g-, -hh-, (D); -m, -m-, (E).
2nd sing: t-, -th, (A); -s, -s-, (B).
3rd sing: -t, -t-, (A); also various demonstrative forms such as i-, s-, t-, k-, (I, B, A, D).

1st dual: n-, (F); m-, (E); -w-, (G).
2nd dual: y-, (H); w-, (G); -t-, (A).
3rd dual: -t-, (A); also above dem. forms: (I, B, A, D).

1sr plur: n-, (F); m-, -m-, (E); w-, -w-, (G).
2nd plur: y-, (H); w-, (G); -t-, (A); s-, (B).
3rd plur: -nt, -nt-, (F, A); -r-, (K); dem: (I, B, A, D).

dem: i-, o-, n-, s-, t-, k-, (I, J, F, B, A, D).

int: k-, (D).

In Collinder's listed works the significant elements of the Common Uralic pronominal forms seem to have been the following:

1st sing: m-, -m-, -m, (E); -k or -g, (D).
2nd sing: t-, -t-, (A).
3rd sing: (Participial base, or demonstratives).

1st plur: m-, -m-, (E); -w-, (G).
2nd plur: t-, -t-, (A).
3rd plur: (Plural of participial base, or demonstratives).

dem: e-, n-, s-, t-, (I, F, B, A).

int: k-, (D); m-, (E).

Common Uralic probably had dual forms, presumably with m and t for the 1st and 2nd pers, respectively, but since these forms do not seem to be well attested no attempt will be made to include them here. The -k or -g in the 1st sing is suggested by certain endings in Selkup and Hungarian and by -g- in Permian languages.

Mainly from the Uralo-Altaica list in 1.1, but partly from 16.1 and 17.1 also, the significant elements in the Common Altaic pronominal forms are taken as

1st sing: b-, (C); m-, (E).
2nd sing: s-, (B); ch- (A) -- (presumably from ti-).

1st plur: b-, (C); m-, (E).
2nd plur: s-, (B); t-, (A).

dem: e-, t-, (I, A).

int: k-, (D); m-, (E).

Most of these elements occurred also in Common Uralic (except 1st pers b- and 2nd pers s-). The demonstrative elements in Altaic may also have included o- and s- or -s-. The first is seen in the 3rd pers pronoun in Turkish. The third may occur in the Turkish 3rd sing pronominal suffix attached to nouns ending in a vowel. However, these elements will not be further considered since they do not seem to be well attested. 2nd pers s- is well attested in Tungusic and Turkic languages but it cannot be cited for Common Uralic since its only clear occurrence there is in Finnish, and Collinder derives Finnish si- from earlier *ti- in this case.

Using the above data, I list the categories common to IndoEuropean and Uralic as follows:

1st sing: D, E. 2nd sing: A. 1st plur: E, G. 2nd plur: A. 3rd pers or dem: I, F, B, A. int: D.

This makes a total of 11 agreements.

The categories common to IndoEuropean and Altaic are:

1st sing: E. 2nd sing: B, A. 1st plur: E. 2nd plur: B, A. 3rd pers or dem: I, A. int: D.

Here there are 9 agreements.

The categories common to Uralic and Altaic are:

1st sing: E. 2nd sing: A. 1st plur: E. 2nd plur: A. 3rd pers or dem: I, A. int: D, E.

Here we count 8 agreements.

The categories common to all three families are:

1st sing: E. 2nd sing: A. 1st plur: E. 2nd plur: A. 3rd pers or dem: I, A. int: D.

This totals to 7 common to all three families.

Thus we have noted 11 agreements between IndoEuropean and Uralic, 9 between IndoEuropean and Altaic, 8 between Uralic and Altaic, and 7 common to all three. Intuitively, these agreements seem too numerous to be accidental. Whether they actually are remains to be seen. Incidentally, in each family the interrogative element k- is differentiated into two forms, one having a front vowel (-e- or -i-) and the other a rounded back vowel (-o- or -u-). It appears, then, that each of the above four numbers ought to be increased by one unit, becoming respectively 12, 10, 9 and 8. However, in order to be conservative, I shall use the unaugmented numbers in this investigation.

THE NON-EURASIATIC PRONOMINAL DATA

In this and succeeding sections I shall try to determine whether the above noted agreements are sufficiently numerous to be regarded as statistically significant. I shall begin by listing what seem to be the significant pronominal elements in each of several non-Eurasiatic languages, each believed to be unrelated to all the others and to each of the Eurasiatic families, and I shall count the number of agreements which each non-Eurasiatic language has with each of the three Eurasiatic families. Unfortunately, interrogative and demonstrative elements for some of the languages are lacking. However, in discussing the data, the demonstrative and 3rd pers elements will be placed in the same functional group, precisely as done with the Eurasiatic families, since many languages use the former to represent the latter. In the following lists, where there are two columns the one on the left gives singular forms; the one on the right, plural forms. Dual forms will be omitted because they occur too sporadically to be useful.

The Maya-Soke family will be represented by Wastec (from 4.1):

1	n, u, (F, J).	u, i, (J, I).
2	t, a, (A, J).	t, a, (A, J).
3	u, n, (J, F).	u, n, (J, F).

IndoEuropean: 2nd sing: A. 2nd plur: A. 3rd pers or dem: J, F.
Total: 4 agreements.

Uralic: 2nd sing: A. 2nd plur: A. 3rd pers or dem: F.
Total: 3 agreements.

Altaic: 2nd sing: A. 2nd plur: A.
Total: 2 agreements.

The Pinutia family will be represented by Northern Sahaptin (from 5.1):

1	*n, c (s' ?), (F, B).*	*n, (F).*
2	*m, (E).*	*m, (E).*
3	*p, (C).*	*p, (C).*

dem: tc, k, y, p, (B, D, H, C).

int: m, t, c, (E, A, B).

IndoEuropean: 1st plur: F. 3rd pers or dem: B, D.
Total: 3 agreements.

Uralic: 3rd pers or dem: B. int: E.
Total: 2 agreements.

Altaic: int: E.
Total: 1 agreement.

The Chirino family will be represented by Candoshi (from 6.1):

1	*i, (I).*	*i, n, (I, F).*
2	*i, sh, (I, A).*	*i, s, (I, B).*
3	*o, (J).*	*a, n, (J, F).*

IndoEuropean: 1st plur: F. 2nd sing: A. 2nd plur: B. 3rd pers or dem: J, F.
Total: 5 agreements.

Uralic: 2nd sing: A. 3rd pers or dem: F.
Total: 2 agreements.

Altaic: 2nd sing: A. 2nd plur: B.
Total: 2 agreements.

3. The Eurasiatic Pronouns and the IndoUralic Question

The Uto-Aztec-Tano family will be represented by Tepecano (from 7.1):

1	*n, (F).*	*t, (A).*
2	*m, p, (E, C).*	*m, p, (E, C).*
3	*a, (J).*	*m, a, (E, J).*

dem: hoba, (D, J).

int: harov, (D).

IndoEuropean: 3rd pers or dem: D, J. int: D.
Total: 3 agreements.

Uralic: int: D.
Total: 1 agreement.

Altaic: int: D.
Total: 1 agreement.

These Sumerian elements were taken from Les Langues du Monde (8.1):

1	*m, (E).*	*m, (E).*
2	*z, (B).*	*z, m, (B, E).*
3	*e, n, b, (I, F, C).*	*e, n, b, (I, F, C).*

dem: hh, e, n, b, (D, I, F, C).

int: aba, mule, ana, (J, C, E, K, F).

IndoEuropean: 1st sing: E. 2nd sing: B. 1st plur: E. 2nd plur: B. 3rd pers or dem: I, D, F.
Total: 7 agreements.

Uralic: 1st sing: E. 1st plur: E. 3rd pers or dem: I, F. int: E.
Total: 5 agreements.

Altaic: 1st sing: E. 2nd sing: B. 1st plur: E. 2nd plur: B. 3rd pers or dem: I. int: E.
Total: 6 agreements.

These Elamite elements likewise are from 8.1:

1	*u, hh, (J, D).*	*niku, elu, hhu, (F, D, I, K).*
2	*n, t, (F, A).*	*num, hhti, (F, J, D, A).*
3	*e, i, (I); s, (A).*	*ape, apu, hhsi, (J, C, D, A).*

IndoEuropean: 1st sing: D. 2nd sing: A. 1st plur: F. 2nd plur: A. 3rd pers or dem: I, A, D, J.
Total: 8 agreements.

Uralic: 1st sing: D. 2nd sing: A. 2nd plur: A. 3rd pers or dem: I, A.
Total: 5 agreements.

Altaic: 2nd sing: A. 2nd plur: A. 3rd pers or dem: I, A.
Total: 4 agreements.

These Andaman elements also are from 8.1:

1	*d, (A).*	*m, (E).*
2	*n, (F).*	*n, (F).*
3	*a, o, (J).*	*ed, od, (I, J, A).*

IndoEuropean: 1st plur: E. 3rd pers or dem: I, J, A.
Total: 4 agreements.

Uralic: 1st plur: E. 3rd pers or dem: I, A.
Total: 3 agreements.

Altaic: 1st plur: E. 3rd pers or dem: I, A.
Total: 3 agreements.

The elements common to the Bantu forms in 8.1 are as follows:

1	*m, (E).*	*t, (A).*
2	*w, (G).*	*n, (F).*
3	*ye, (H, I).*	*b, (C).*

IndoEuropean: 1st sing: E. 3rd pers or dem: I.
Total: 2 agreements.

Uralic: 1st sing: E. 3rd pers or dem: I.
Total: 2 agreements.

Altaic: 1st sing: E. 3rd pers or dem: I.
Total: 2 agreements.

The Malay-Polynesian family will be represented by Samoan (from 9.1):

1	*'ou, a'u, 'ita, (D, J, I).*	*tatou, matou, (A, E).*
2	*'oe, e, (D, I).*	*oulua, lua, (K,J).*
3	*ia, na, (I, F).*	*latou, (K).*

dem: le, (K).

int: ai, (J).

IndoEuropean: 1st sing: D. 1st plur: E. 3rd pers or dem: I, F, K.
Total: 5 agreements.

Uralic: 1st sing: D. 1st plur: E. 3rd pers or dem: I, F.
Total: 4 agreements.

Altaic: 1st plur: E. 3rd pers or dem: I.
Total: 2 agreements.

In 8.1 the Sudan-Guinea "family" is divided into some 12 groups, each consisting of several languages whose listed pronominal forms vary considerably within the group. This appears to be due, in part, to the prefixing of non-significant phonemes. Further, there is much variation from group to group, some of which seem to resemble Bantu in their pronominal forms. The best I can do, then, is to arbitrarily choose one of the groups which appears to be unrelated to Bantu and to glean from it the significant elements as best I can. I shall take the second group, Nile-Abysinean:

1	*a, (J).*	*a, (J).*
2	*i, (I).*	*u, (J).*
3	*e, (I).*	*e, (I).*

IndoEuropean: 3rd pers or dem: I.
Total: 1 agreement.

Uralic: 3rd pers or dem: I.
Total: 1 agreement.

Altaic: 3rd pers or dem: I.
Total: 1 agreement.

These Burushaski elements are from 10.1:

1	a-, j-, -m, (J, A, E).	m-, -n, (E, F).
2	g-, -ma, (D, E).	m-, -man, (E).
3	e-, m-, -mi, (I, E).	o-, -man, (J, E).

dem: k-, g-, (D); i-, e-, (I).

int: m-, b-, (E, C).

IndoEuropean: 1st sing: E. 1st plur: E, F. 3rd pers or dem: I, D, J.
Total: 6 agreements.

Uralic: 1st sing: E. 1st plur: E. 3rd pers or dem: I. int: E.
Total: 4 agreements.

Altaic: 1st sing: E. 1st plur: E. 3rd pers or dem: I. int: E.
Total: 4 agreements.

These Hebrew elements are from 11.1:

1	e-, -i, -ti, -ni, (I, A, F).	n-, -nu, (F).
2	t, (A).	t, (A).
3	y-, t-, h-, (H, A, D).	y-, t-, h-, (H, A, D).

dem: z-, h-, 'elleh, (B, D, I).

int: m-, (E).

IndoEuropean: 2nd sing: A. 1st plur: F. 2nd plur: A. 3rd pers or dem: I, B, A, D.
Total: 7 agreements.

Uralic: 2nd sing: A. 2nd plur: A. 3rd pers or dem: I, B, A.
int: E.
Total: 6 agreements.

Altaic: 2nd sing: A. 2nd plur: A. 3rd pers or dem: I, A.
int: E.
Total: 5 agreements.

These Dravidian elements are from 12.1:

1	en, (I, F).	am, (J, E).
2	i, (I).	i, (I).
3	av, (J, C).	

dem: i, u, a, (I, J).

int: e, y, (I, H).

IndoEuropean: 1st plur: E. 3rd pers or dem: J, I.
Total: 3 agreements.

Uralic: 1st plur: E. 3rd pers or dem: I.
Total: 2 agreements.

Altaic: 1st plur: E. 3rd pers or dem: I.
Total: 2 agreements.

These Basque elements are from 13.1:

1	n-, -t, (F, A).	g-, (D).
2	h-, z-, -k, -n, (D, B, F).	z-, (B).
3	d-, b-, (A, C).	d-, b-, (A, C).

dem: h-, (D).

int: n-, (F).

IndoEuropean: 2nd sing: B. 2nd plur: B. 3rd pers or dem: A, D.
Total: 4 agreements.

Uralic: 3rd pers or dem: A.
Total: 1 agreement.

Altaic: 2nd sing: B. 2nd plur: B. 3rd pers or dem: A.
Total: 3 agreements.

STATISTICAL APPENDIX II
(Reprint cont.)

An A Posteriori Calculation of Pronominal Agreements

ANALYSIS OF THE DATA

Let us list the noted number of agreements and related data corresponding to the several quasi-alien languages. There are 14 of them, as follows:

i		ki	xim	xi	ui	yim	yi	vi	zim	zi	wi
1	Wastec	10	10	4	.40000	10	3	.30000	10	2	.20000
2	NSahap.	12	12	3	.25000	12	2	.16667	12	1	.08333
3	Candoshi	9	9	5	.55556	9	2	.22222	9	2	.22222
4	Tepecano	10	10	3	.30000	10	1	.10000	10	1	.10000
5	Sumerian	14	14	7	.50000	12	5	.41667	12	6	.50000
6	Elamite	17	17	8	.47059	12	5	.41667	12	4	.33333
7	Andaman	7	7	4	.57143	7	3	.42857	7	3	.42857
8	Bantu	7	7	2	.28571	7	2	.28571	7	2	.28571
9	Samoan	13	13	5	.38462	12	4	.33333	12	2	.16667
10	Nile-Ab.	6	6	1	.16667	6	1	.16667	6	1	.16667
11	Burush.	14	14	6	.42857	12	4	.33333	12	4	.33333
12	Hebrew	13	13	7	.53846	12	6	.50000	12	5	.41667
13	Dravid.	11	11	3	.27273	11	2	.18182	11	2	.18182
14	Basque	11	11	4	.36364	11	1	.09091	11	3	.27273
				--	-------		--	-------		--	-------
Totals:				62	5.48798		41	3.94257		38	3.69105
				X	U		Y	V		Z	W

Because of the limitations of my printing arrangements, I have used the suffix -i along the title row to represent a subscript which designates the i-th member of the associated class. The additional suffix -m represents the maximum value which the i-th member can have.

Here ki is the number of opportunities for agreement offered by the i-th of the 14 quasi-alien languages. The corresponding numbers offered by IndoEuropean, Uralic and Altaic are 19, 12 and 12, respectively. xim is the maximum possible number of agreements which the i-th language can have with IndoEuropean, yim with Uralic, and zim with Altaic. xim is identical to ki since none of the k's is greater than 19; however, yim and zim are each limited to a maximum value of 12 although some of the k's are greater than 12.

xi is the observed number of agreements of the i-th language with IndoEuropean, yi with Uralic, and zi with Altaic. I call these the "raw" or "unnormalized" data.

ui is the RATIO of the observed number of agreements of the i-th language with IndoEuropean (xi) to the maximum possible number of agreements with IndoEuropean (xim); vi is the ratio of yi to yim for Uralic; and wi is the ratio of zi to zim for Altaic. I call the u's, v's and w's the "normalized" data.

For convenience, let us use the following symbolism:

X = the sum of the x's. XX = sum of squares of the x's.
Y " " " " y's. YY " " " " " y's.
Z " " " " z's. ZZ " " " " " z's.
U " " " " u's. UU " " " " " u's.
V " " " " v's. VV " " " " " v's.
W " " " " w's. WW " " " " " w's.

Using these symbols, we apply the following formulas:

$mr = R/n$ .... (sample mean of the r's).
$Mr = RR/n$ .... (mean of the squares of the r's).
$sr2 = Mr - mr*mr$ .... (sample variance).
$Sr2 = n*sr2/(n - 1)$ .... (unbiased variance estimate).
sr = square root of sr2.
Sr = square root of Sr2.

where r = x, y, z, u, v or w, and R = X, Y, Z, U, V or W, as applicable. The asterisk implies multiplication of the quantities adjacent to it.

My method of analysis may most easily be explained by illustrating with a specific example. Thus we can start with the u's, the normalized numbers of agreements of the 14 languages with IndoEuropean. Applying the formulas, we get mu = 0.391999 and Su = 0.125705

The observed number of agreements of IndoEuropean with Uralic is 11, and the possible number of agreements is 12; the ratio of these numbers is 11/12 or 0.916667, which I shall call IE.U# (using the pound sign to indicate that this value is based on normalized data). This is the normalized number of agreements of IndoEuropean with Uralic.

Now let us imagine that our group of 14 languages is allowed to grow to an indefinitely large number (many millions, say, as if such a large number of mutually alien languages could actually exist on this planet); our set of 14 u's grows correspondingly, one u for each language. I continue to assume that the pronominal systems of all these languages developed independently of one another, and likewise independently of each of the pronominal systems of the three Eurasiatic languages. Accordingly, the u's in the enlarged set would be independent of one another and of the datum IE.U#, just as is believed to be true of the actual u's in the set of 14. By applicability of the

Central Limit Theorem, we can assume that the u's in the enlarged set have an approximately Gaussian distribution, centered about a specific mean value with a specific variance. Our measured values of mu and Su2 are (respectively) unbiased estimates of the true (or "universe") mean, and the variance of the u's about the true mean. Here "true" refers to the values that would be observed if the number of u's were to increase without limit. Our problem is to determine whether IE.U# may reasonably be regarded as a member of the enlarged set of u's.

After all, if the null hypothesis is correct, then IE.U# is indeed a member of the enlarged set and is unlikely to differ greatly from mu, for it was generated by the same process of comparison as all the other u's, whether real or hypothetical. It would then be reasonable to expect, under the null hypothesis, that a considerable proportion of the u's in the enlarged set (more than five percent, say) would be greater than IE.U#; hence we can expect that fewer than 95 percent would be less than this value. On the other hand, if our test shows that LESS than five percent of the u's in the enlarged set should exceed IE.U#, this weight of evidence will be regarded as "significant" and I shall reject the hypothesis that IE.U# is a member of the enlarged set; I would interpret this to mean that most of the pronominal agreements of IndoEuropean with Uralic did not come into being by independent developments but instead were CAUSED by some UNIQUE historical circumstance.

It may be noted that we now have two samples taken from the universe of potential u's. One consists of the control sample of 14 u's; the other consists of the single datum IE.U#. Of course, the existence of the latter as a valid sample is contingent on the correctness of the null hypothesis. The mean of this sample is equal to the datum itself, and its sample variance is zero.

It is my intent to test the null hypothesis by first tentatively accepting it so that the two samples may provisionally be regarded as taken from the same universe of potential u's, and then comparing the two samples on the basis of that assumption. If the means of the two samples are reasonably alike, I shall be compelled to accept the null hypothesis, concluding that the agreements evolved by independent developments. On the other hand, if the two means prove to be sufficiently unlike, I shall reject the null hypothesis and conclude that most of these agreements were caused by some unique historical circumstance.

A comparison on the basis of sample means seems particularly appropriate in this case because the mean of one of the samples happens to be identical to its single datum IE.U#; hence the result of the comparison will be directly relevant in determining whether this datum ought to be regarded as a member of the enlarged set of potential u's, which is exactly what we wish to determine. It is appropriate to use a t-test since the samples are small.

The proper statistic to use is

$$t = [(ma - mb) - (ha - hb)]/[S*sqr(1/na + 1/nb)] \qquad (1)$$

where S is the square root of a weighted unbiased variance estimate given by

$$S2 = (na*sa2 + nb*sb2)/(na + nb - 2) \qquad (2)$$

The sqr(---) symbol in (1) represents the square root of the parenthesized quantity; as before, the asterisk implies the multiplication of adjacent quantities. The "degrees of freedom" is DF = (na + nb - 2). In this discussion the suffix -a refers to one of the samples and the suffix -b refers to the other. The m's are the sample means, the h's are the true means of the parent populations from which the samples were respectively drawn, the n's are the sample sizes, and the s's are the square roots of the sample variances. In the present case, under the null hypothesis, both samples are assumed to have been drawn from the same population (the set of potential u's); hence the true means are identical by definition, so ha = hb and (ha - hb) = 0. Also, using the suffix -a to refer to the smaller sample and the suffix -b to refer to the larger, we have ma = IE.U#, mb = mu, na = 1, nb = 14, sa2 = 0, and sb2 = su2. Making these substitutions, we get

$$t = (IE.U\# - mu)/[Su*sqr(1 + 1/14)] \qquad (3)$$

The degrees of freedom is DF = (1 + 14 - 2) = 13. Now substituting the values IE.U# = 0.916667, mu = 0.391999, and Su = 0.125705, we get t = 4.0323. For a DF of 13 this yields a "one-tailed" probability of P = 0.000711, which is less than even a tenth of one percent. This result is very highly significant, refuting the null hypothesis most emphatically. We have to conclude that it is extremely unlikely that the pronominal systems of IndoEuropean and Uralic evolved independently.

Our use of a "one-tailed" test is justified by the fact that we are concerned with only the upper tail of the t-distribution, for any consideration of the lower tail is eliminated by the fact that if IE.U# were less than mu the null hypothesis would be confirmed automatically, regardless of any other consideration.

Repeating the test with normalized Uralic data, we have

$$t = (U.IE\# - mv)/[Sv*sqr(1 + 1/14)] \qquad (4)$$

where U.IE# = 11/12 = 0.916667 (the normalized number of agreements of Uralic with IndoEuropean, = IE.U#), mv = 0.281612, and Sv = 0.130306. Using these values, we get t = 4.7083, yielding P = 0.000205. This is even more significant than before, again emphatically refuting the null hypothesis.

Repeating with unnormalized IndoEuropean data, we have IE.U = 11 (the absolute, or "unnormalized", number of agreements of IndoEuropean with Uralic), mx = 4.42857, and Sx = 2.02729, getting t = 3.1316. This yields P = 0.00397. Although this value for P is an order of magnitude greater than observed before, it still is less than one percent, thus still indicating high significance and again emphatically refuting the null hypothesis.

Again repeating with unnormalized Uralic data, we have U.IE = 11, my = 2.92857, and Sy = 1.63915, getting t = 4.7572. This yields P = 0.000187, very highly significant and again emphatically refuting the null hypothesis.

Thus, regardless whether we test with IndoEuropean or Uralic data, or whether the data are normalized or not, the results are uniformly and highly significant, repeatedly and emphatically refuting the null hypothesis. The only reasonable conclusion is that the pronominal agreements between Indo-European and Uralic are not due to independent developments, but instead were CAUSED by some UNIQUE historical circumstance. Clearly, this conclusion is of direct concern to anyone who is interested in the IndoUralic hypothesis.

Similarly, those who are interested in the UralAltaic hypothesis will be concerned with the results of testing the agreements of Uralic with Altaic, as follows:

Testing with the normalized Uralic data, we have U.A# = 8/12 = 0.666667 (the normalized number of agreements of Uralic with Altaic), mv = 0.281612, and Sv = 0.130306, getting t = 2.8548. This yields P = 0.00677. Since this is less than one percent, the result is highly significant, emphatically refuting the null hypothesis.

Repeating with normalized Altaic data, we have A.U# = 8/12 = 0.666667 (the normalized number of agreements of Altaic with Uralic), mw = 0.263646, and Sw = 0.126286, getting t = 3.0831. This yields P = 0.00436. Since this is less than one percent, the result again is highly significant, again emphatically refuting the null hypothesis.

Repeating with unnormalized Uralic data, we have $U.A = 8$ (the absolute, or "unnormalized", number of agreements of Uralic with Altaic), $my = 2.92857$, and $Sy = 1.63915$, getting $t = 2.9890$. This yields $P = 0.00523$. As before, this is highly significant, again emphatically refuting the null hypothesis.

Repeating with unnormalized Altaic data, we have $A.U = 8$ (the unnormalized number of agreements of Altaic with Uralic), $mz = 2.71429$, and $Sz = 1.54066$, getting $t = 3.3145$. This yields $P = 0.00279$. Again, this is highly significant, again emphatically refuting the null hypothesis as in the previous three tests.

Thus, regardless whether we test the Uralic-Altaic agreements with Uralic or Altaic data, or whether the data are normalized or not, the results are uniformly and highly significant, always emphatically refuting the null hypothesis. This means that the pronominal agreements between Uralic and Altaic did not develop independently, but instead were CAUSED by some UNIQUE historical circumstance.

Likewise, those who are concerned with the IndoAltaic hypothesis will be interested in testing the agreements between IndoEuropean and Altaic, as follows:

Testing with normalized IndoEuropean data, we have $IE.A\# = 9/12 = 0.75$ (the normalized number of agreements of IndoEuropean with Altaic), $mu = 0.391999$, and $Su = 0.125705$, getting $t = 2.7514$. This yields $P = 0.00825$. Since this is less than one percent, the result is highly significant.

Repeating with normalized Altaic data, we have $A.IE\# = 9/12 = 0.75$, $mw = 0.263646$, and $Sw = 0.126286$, getting $t = 3.7206$. This yields $P = 0.00128$. Again the result is highly significant.

Repeating with unnormalized IndoEuropean data, we have $IE.A = 9$ (the absolute, or "unnormalized", number of agreements of IndoEuropean with Altaic), $mx = 4.42857$, and $Sx = 2.02729$, getting $t = 2.1785$. This yields $P = 0.0242$. This is greater than one percent, so the result is not "highly" significant. However, it is less than five percent, so the result is still "significant".

Repeating with unnormalized Altaic data, we have $A.IE = 9$, $mz = 2.71429$, and $Sz = 1.54066$, getting $t = 3.9415$. This yields $P = 0.000844$. This is very highly significant.

Thus, regardless whether we test the IndoEuropean-Altaic agreements with IndoEuropean or Altaic data, or whether the data *are normalized or not, the results are highly significant in every case except when testing with unnormalized IndoEuropean data, and even here P is less than five percent. This means that the pronominal agreements between IndoEuropean and Altaic did not develop independently, but instead were CAUSED by some UNIQUE historical circumstance.*

CONCLUSIONS: The results are overwhelming. We are forced to conclude that the pronominal agreements between IndoEuropean and Uralic, between Uralic and Altaic, and between IndoEuropean and Altaic, did not develop independently, but instead were CAUSED by some UNIQUE historical circumstance. In short, it is extremely unlikely that the three pronominal systems could have evolved independently.

***[(1982 COMMENT: An inspection of the above group of 14 quasi-alien languages will disclose that at least four of them have a surprisingly large number of agreements with the three Eurasiatic languages, especially IndoEuropean. These are Sumerian, Elamite, Burushaski and Hebrew. Hebrew, of course, is a member of the Semitic branch of AfroAsiatic (or HamitoSemitic); the other three are generally regarded as "genetically" isolated. For geographical reasons, it seems not unreasonable to suspect that all of them, together with the three Eurasiatic families, may have had a common origin in the region around the Caucasus and the Caspian Sea provided we go far enough back into time, say the Upper Paleolithic era. If this is true, it would provide a plausible explanation for the relatively large number of observed agreements. On the other hand, it would violate the requirement that the 14 languages be unrelated, both among themselves and with each of the three undivided Eurasiatic languages.*

In this connection, Collinder's 1974 publication is particularly important. He refers to recent work by Soviet scholars, led by Dolgopol'ski, apparently showing a "genetic" relationship among IndoEuropean, Uralic, Altaic, certain PaleoSiberian languages (Chukchi, Koryak, Kamchadal), Caucasian languages (Kartvelian, Georgian), and AfroAsiatic. Their colleague, Illich-Svitych, who unfortunately died prematurely, made a similar study which included Dravidian; he reached similar conclusions. This of course reminds us of the fact that a number of scholars, for many years, have been concerned with the possibility of the existence of a "Nostratic" linguistic family believed to include most of these languages, particularly AfroAsiatic along with Altaic, Uralic and IndoEuropean. Also, most speci-

alists in Dravidian seem to be convinced that this family is related to Uralic and Altaic.

At the time I wrote my pamphlet I was unconcerned about the "Nostratic" hypothesis since I knew too little about its supporting evidence to justify my holding an opinion. Looking back on it, I concede that this may have been a mistake; in principle, it is the same mistake that IndoEuropeanists make in ignoring the implications of the IndoUralic connection. In this case, my mistake consisted in including some languages which may well be related to the Eurasiatic group, particularly Hebrew and Dravidian. Accordingly, I have recently modified my analysis by repeating it with these languages excluded. Sumerian, Elamite and Burushaski were likewise excluded since they invite suspicion by virtue of their historic proximity to the Caucasus. Ideally, I should compensate for their omission by introducing new languages taken from farther afield, but I will leave that to others.

Thus, in the new analysis, the 14 languages are reduced to 9, and the degrees of freedom (DF) to 8 instead of 13 as in the previous calculations. This could result in a complication due to the small sample size. It is quite true that the t-distribution is designed to compensate for small sample size, and it does this very well. But when we reduce the sample size the DF is reduced correspondingly, and the associated probability density function is lower at the center and consequently higher in the "tails". The result is that it is more difficult to refute the null hypothesis since the two means have to be more widely separated in order to establish a given level of significance.

The results of the new analysis are here listed in comparison with the old:

TEST	DATUM	t(13)	P(13)	t(8)	P(8)
IE v U	IE.U#	4.0323	0.000711	3.9120	0.00223
U v IE	U.IE#	4.7083	0.000205	5.7362	0.000218
IE v U	IE.U	3.1316	0.00397	5.3759	0.000332
U v IE	U.IE	4.7572	0.000187	8.0000	0.0000218
U v A	U.A#	2.8548	0.00677	3.6396	0.00330
A v U	A.U#	3.0831	0.00436	4.0614	0.00181
U v A	U.A	2.9890	0.00523	5.3000	0.000364
A v U	A.U	3.3145	0.00279	7.4162	0.0000375
IE v A	IE.A#	2.7514	0.00825	2.7319	0.01288
A v IE	A.IE#	3.7206	0.00128	4.8090	0.000670
IE v A	IE.A	2.1785	0.02418	3.9528	0.00211
A v IE	A.IE	3.9415	0.000844	8.6298	0.0000126

These results are listed in the same order as given in my pamphlet. As before, the suffixed pound sign (-#) indicates that the normalized data were used in the associated calculation. The third and fourth columns merely repeat the t and P values obtained in the old study (with DF = 13), while the fifth and sixth give the values obtained in the new (with DF = 8). It is clear that the overall results are unchanged -- we have high significance in almost every case; if anything, most of the P's seem to be actually smaller than before.

At first sight this would seem surprising in view of the fact that the new study involved a lower DF (8 instead of 13), where high levels of significance are usually difficult to attain since the means have to be more widely separated. However, in most of the present calculations this seems to have been more than compensated by the fact that four of the five eliminated languages showed relatively high numbers of agreements with the Eurasiatic languages, resulting in a generally increased separation of means when they were eliminated; this is not surprising in view of the fact that we have reason to suspect their non-independence. In other words, their large numbers of agreements may be the result of a common prehistoric inheritance of their pronominal systems.

This illustrates the importance of trying to carefully screen the languages which may be under consideration for inclusion into the control sample. Nevertheless, in these studies the available data run so strongly against the null hypothesis that the overall result is not substantially affected despite any errors that I may have made.

This also illustrates the fact that it is not necessary that the data in the control sample be ABSOLUTELY independent, but merely that the DEGREE of their independence be enough to result in statistical significance; it implies a significant difference in the historical circumstances. -- JCKerns)]

End of Reprint of

The Eurasiatic Prounouns and the Indo-Uralic Question

4. NOTES on Collinder's INDO-URALISCHES SPRACHGUT

***[(1982 NOTE: Because of its seminal importance, this work by Collinder is here presented, much condensed, in the form of notes in skeleton outline. It should always be borne in mind that, in the original, Collinder thoroughly buttressed his comparisons with an abundance of examples from many Uralic and IndoEuropean languages. At the same time, it should be noted that examples from Samoyedic languages were lacking in the instances of a number of morphological formants, but in almost every case the lack was later rectified in his Comparative Grammar of the Uralic Languages (1959). The reason for this is that the comparative grammar of the Samoyedic languages was still undeveloped at the time the Indo-uralisches Sprachgut was written.*

I hope that my readers will be incited to study the original with great care and reflection, which it clearly merits.

The title means "IndoUralic Speech Material", data relevant to the IndoUralic question.

A special comment is added at the end. -- JCKerns)]

Collinder begins with his VORWORT (Foreword), saying that most of the work which led to this investigation was done in the years 1930-1932, which in turn was based mainly on work done during his stay at Helsingfors (Helsinki) in the winter of 1921-1922.

In this investigation he did not expect to prove the Indo-Uralic hypothesis, but only to show that the counter-arguments that had thus far been brought against it had no sound basis, and that we are thus justified -- and even obliged -- to make use of that hypothesis as a good working assumption. Other hypotheses concerning linguistic relationships would not be discussed; he would only express the view that of all the hypotheses of like nature thus far proposed -- the UralAltaic hypothesis not excepted -- the IndoUralic hypothesis was best authenticated.

This investigation would be on a purely linguistic basis, making no reference to anthropology, archeology or ethnology.

He dedicated this work to his friend and teacher, K. B. Wiklund. Date: 24 Nov 1933.

INDO-EUROPEAN PREHISTORY

EINLEITUNG (Introduction)

The first to put the question concerning the existence of a "genetic" connection between the FinnoUgric linguistic family and IndoEuropean on a firm scientific basis was Vilhelm Thomsen. In the Introduction to his work on the influence of Germanic languages on Finnish-Lappish he mentioned four word correspondences which could imply a "genetic" connection; these included Fi. nime- (name) and vete- (water). He concluded, however, that the structure of the languages in the two groups was so radically different that a "genetic" relationship should be ruled out. Nevertheless, in his later years he changed his mind and firmly supported the IndoUralic hypothesis.

In 1879 the Estonian scholar, Nicolai Anderson, published his Study on a comparison between the IndoEuropean and Finno-Ugric languages, an especially valuable work, in which he presented an abundance of material evidence. Everything that was asserted in later years, for and against, derived mainly from Anderson's achievement. Some have branded his masterwork as outmoded and lacking in method, but unjustifiably. Indeed there was some lack of method in that he failed to take sufficient account of loanwords. In other respects, however, his book displays great methodical excellence. And Ojansuu emphatically remarked that Anderson's work in its overall influence on later FinnoUgric investigations has lost none of its cogency.

Budenz found Anderson's work premature and lacking in importance. He considered Anderson's grammatical comparisons too dissimilar in sound, meaning and syntax to be regarded as convincing. In regard to the lexical comparisons, he deemed that these could not be cognates because of fundamental differences in the phonetic systems, which he considered an insuperable objection. But here he overlooked that fact that, historically, languages have been known to change in seemingly fundamental ways, both phonetically and otherwise. Hence the differences which he considered crucially important are not necessarily so, and can plausibly be accounted for as secondary developments.

Munkacsi, in the introduction to his great work on the Aryan and Caucasian constituents in FinnoUgric languages, made detailed references to Anderson's Study and raised serious objections against Budenz. Budenz stated, for example, that the pronominal agreements between the two families were not significant. But Munkacsi correctly asserted that when comparison is made with non-Eurasiatic languages the number of agreements is significantly less.

Among the IndoEuropeanists, Henry Sweet and Holger Pederson spoke out in favor of Anderson's position, the first in a much different manner from that of Anderson himself.

4. NOTES on Collinder's Indo-uralisches Sprachgut

In 1905 K. B. Wiklund published a work in which he presented a critical review of both the FinnoUgric scholarly publications of the previous era and the reactions of the routine investigators of loan words. He expressed an objection to the usual view that the Uralic languages have a structure which is significantly different from that of the IndoEuropean languages; he correctly stated that this view represented, at least in part, a certain "mysticism" in the thinking of linguistic investigators. It should be remarked, in passing, that Budenz in his investigation of the grammatical structure of the languages in the two groups saw no argument against their "genetic" connection. Wiklund took into account the fact that Samoyedic together with FinnoUgric comprise the Uralic family.

The Hungarian IndoEuropeanist, Joseph Schmidt, presented a publication which started a polemic with Wiklund. He expressed the view that the comparative data which had thus far been presented were neither in quantity nor in quality such as to justify a firm position on the IndoUralic question, that there seemed to be no way to resolve the matter with finality, and that it seemed unlikely that a way would soon become apparent. Like Budenz, he ascribed the grammatical similarities and the few lexical agreements to chance, and the pronominal agreements to borrowing from IndoEuropean. He ascribed special importance to the fact that the two groups do not have common words for such primitive concepts as parts of the body and family relationships, but he overlooked the fact that the terms are not uniform within each family separately. Schmidt put special emphasis on the fact that the two families do not have common numerals, but this too is not significant.

Wiklund did not overlook this difficulty. He showed that within Uralic itself there was a general lack of common numerals, especially between Samoyedic and FinnoUgric, and since these two subgroups are undeniably "genetically" related it follows that the possession of a common fund of numerals is not a necessary criterion of relatedness. Further, cognates of numerals in the wider sense apparently do exist between Uralic and IndoEuropean. These include Fi. paljo- and luke-, both attested for Uralic generally, and which seem to be cognate to Greek polu's and logos.

In 1907 Heikki Paasonen published an article which was particularly interesting in regard to method, in which he expressed a benevolent skepticism toward the IndoUralic hypothesis. Yet even in that work he provided a considerable number of new word comparisons. He further stated that the increase in word correspondences naturally makes an explanation on the basis of chance less likely.

Among IndoEuropeanists, Hirt and Schrader changed their minds in favor of the IndoUralic hypothesis after reading

Wiklund's work, and the same appears to be the case with the FinnoUgrist Setaelae, who in his later years provided some new word comparisons. Ojansuu was already of the same opinion.

In 1926 Hannes Skoeld published an article which, while it contained no new material, nevertheless greatly advanced the investigation. Skoeld's article, which focused on lexical comparisons, and especially vocalic criteria, was perhaps premature to show the vocalic relationships, which he himself said was an unknown territory. He was the first to get evidence from Hittite. One of his achievements was to show that the three-gender system of IndoEuropean was an innovation and hence cannot be used as an argument against the IndoUralic hypothesis.

More recently, Kieckers and Mladenov came out in favor of the IndoUralic hypothesis.

Hermann Jacobsohn has adopted a skeptical attitude, which he got from Ernst Levy, who showed similarities between Finnish and some Indian languages in order to ridicule the IndoUralic hypothesis. It should be remarked that such contrary attempts always serve to provide a warning, at the very start, that a rigidly consistent method must be used.

The IndoUralic question was debated at the II International Linguistic Congress in Geneva, but no conclusion was reached. In 1933 it was again debated at the Congress in Rome, yielding the conclusion that there are similarities which can most easily be explained as due to "genetic" relationship. In the same attitude Holger Pedersen treated the problem of "genetic" relationship between large linguistic groups and especially sought to strengthen the IndoUralic hypothesis by focusing on problems of declension.

Collinder's investigation brings both morphological and lexical similarities into prominence and provides more material than his predecessors, and the lexical material is more critically examined in order to exclude loanwords from various Post-IndoEuropean languages. Research during the 1920's had shown that there had been a considerable number of loanwords in various Post-FinnoUgric languages, and earlier in Common FinnoUgric also, from various Post-IndoEuropean languages, but no trace of them in Common Uralic, despite Sauvageot. Therefore, so far as the purely lexical material is concerned, he includes only those word correspondences which are represented in Samoyedic as well as FinnoUgric. Thus the question as to whether these Common Uralic words are loans from IndoEuropean does not arise. At the same time, he recognizes that there may well be a number of IndoUralic cognates which are unrecognizable because of accrued phonetic changes.

4. NOTES on Collinder's Indo-uralisches Sprachgut

MORPHOLOGICAL AGREEMENTS

PLURAL

-i, -i-. Used to designate the plural of nouns in oblique cases in Uralic (including Samoyedic) and IndoEuropean; in both families the -i- precedes the case ending in this application. Also used in the nominative plural of pronouns in both families.

CASE ENDINGS

At first sight, the large number of case endings (some 16 to 21) found in certain FinnoUgric languages seems to exclude the possibility of a "genetic" connection between the case endings in the two groups since only about 8 separately distinguished cases are attributed to Common IndoEuropean as ordinarily reconstructed. Therefore, even if the two families actually are ultimately "genetically" related, it would be natural to expect that there would be no discernible vestige of common case endings at this late date.

On closer examination, however, this is found to be untrue. In the first place, it is quite clear that most of the local cases in Finnish and Hungarian are of separate and hence secondary origin, and when comparison is made with the other Finno-Ugric languages it is apparent that only six or seven can be attributed to Common FinnoUgric. And when further comparison is made with the Samoyedic languages it becomes apparent that Common Uralic had the following case endings in the singular:

Nominative: (endingless like the neuter in IE)
Genitive: -n (same as in Altaic)
Dative-Lative: -n'V (palatal n followed by a front vowel)
Accusative: -m (same as in IE)
Locative: -na
Ablative: -ta and -dha (much as in Altaic and IE)

**[(1982 NOTE: This is of course a minimum list of the case endings which seem to be attributable to Common Uralic; there may have been others, but we have no way of knowing what they were, or even if they existed. In this list Collinder and others have identified the -m accusative and the -ta ablative as similar in form and function to those in IndoEuropean -- and the ablative also in Altaic, at least in earlier times -- JCKerns)]

CASE ENDINGS, Cont.

*Casus Rectus (non-oblique cases) in -t or -t+vowel. This has been used as an ending for the nominative-accusative singular of pronouns. On the Uralic side it is especially prominent in the nominative of demonstrative pronouns in Lappish; and in other Uralic languages, especially Samoyedic, it appears with an additional vocalic suffix. On the IndoEuropean side it is represented by the final consonant in *id, *tod, and *kwid or *kwod. The fact that, in IndoEuropean, this ending occurs only in neuters, merely illustrates the fact that the neuter was the oldest of the IE genders, from which the others were secondarily derived.*

OBLIQUE CASES WITH -N- STEM

In IndoEuropean (and also in the Mongol branch of Altaic) this feature is well represented; it appears in the neuter r-n declension where the stem differentiation emphasizes the dichotomy between casus rectus (nominative, or nominative-accusative) and oblique; apparently it occurs also in n-stems of common gender (masculine or feminine) as illustrated by Latin homo, hominis. In Uralic languages it is less well represented; here it appears to be an ancient formation which is vestigially preserved as a stem for personal possessive endings for nouns in oblique cases.

COMPARISON

The only comparative suffix in Uralic languages comparable to any in IE is -m- or -mp-. In IUS (Indo-uralisches Sprachgut) it is attested only in FinnoUgric, but in his Comparative Grammar (see 1.2, pp 260-261) Collinder cites it for Samoyedic as well, thus attesting it for Uralic as a whole.

VERBAL FORMANTS

Common present stem formants are -y- (or -i-), -n- and -sk-. For the preterit there are -y- (or -i-) and palatal -s'-, the former being attested only in FinnoUgric. There is reason to suspect, however, that these formants are historically identical to nominal formants in both Uralic and IndoEuropean. As such, all of them are attested in Samoyedic as well as FinnoUgric; hence they are significant for IndoUralic.

4. NOTES on Collinder's *Indo-uralisches Sprachgut*

PERSONAL ENDINGS of VERBS

Common endings are -m- for 1st person singular and plural, and -t- for 2nd person singular and plural. In both families these are related to personal pronouns, which explains the similarity since the pronominal roots are largely the same.

STEM BUILDING

*-y, -y- or -i, -i-, both denominal and deverbal. This is well attested in both IndoEuropean and FinnoUgric, but Collinder shows nothing from Samoyedic. **[(In 1.2, p 257, Collinder does cite examples from Samoyedic.)]*

-kk-, denominal only. This is well attested in IE and in both branches of Uralic. It is used to form diminutives and similar derivatives from nouns and adjectives.

*-l(V), denominal. Well attested in IE and FU, but Collinder cites nothing from Samoyedic. **[(In 1.2, p 260, Collinder does cite examples from Samoyedic.)]*

-m(V), denominal and deverbal formant. It is well attested in IE and in both branches of Uralic, and even in Altaic languages. It is used to form verbal nouns and adjectives, infinitives and participles. It has an extension, -men-, attested in IE and Finnish.

*-n-, deverbal and denominal, well attested in IE and FU, but Collinder shows nothing from Samoyedic. **[(In 1.2, p 262, Collinder does show examples from Samoyedic.)]*

*-nt-, deverbal, forming derivative nouns and participles from verbs, attested in IE and FU, but not in Samoyedic. **[(In 1.2, p 262, Collinder does show probable examples from Samoyedic.)]*

*-p(V)-, -bh(V)-, deverbal and denominal, forming derivative nouns, adjectives and participles. It is attested in IE and FU but not in Samoyedic. **[(In 1.2, p 263, Collinder does show examples from Samoyedic.)]*

*-r-, denominal, forming derivative nouns from simpler nouns, attested in IE and FU but not in Samoyedic. **[(In 1.2, p 264, Collinder does show examples from Samoyedic.)]*

-s-, or -s'-, deverbal, forming abstract and agent nouns from verbs and adjectives, attested in IE and FU but not in Samoyedic. **[(Examples from Samoyedic in 1.2, p 171.)]

-t-, -dh-, both deverbal and denominal, the former attested for IE and FU, the latter for IE and Uralic. The deverbal forms infinitives and participles from verbs; the denominal forms derivatives from nouns and adjectives and is attested in Samoyedic as well as FU and IE.

VERBAL EXTENSIONS

-y-, forms new verbs of specialized meaning from other verbs, and forms verbs from nouns and adjectives, attested in IE and FU but not in Samoyedic. **[(Examples from Samoyedic in 1.2, p 272.)]

-n-, forms inchoative verbs in IE and FU, attested in IE and FU but not in Samoyedic. **[(Examples from Samoyedic in 1.2, pp 273-4.)]

-s'k-, forms inchoative verbs in IE proper, but frequentative verbs in Hittite and FU, not attested in Samoyedic.

SUMMARY COMMENTS on MORPHOLOGICAL AGREEMENTS

Several of these morphological agreements cited by Collinder in his Indo-uralisches Sprachgut were attested only for IndoEuropean and FinnoUgric; some were not attested by him for Samoyedic. As shown in the comments above, however, later work summarized by him in his Comparative Grammar has made good the lack in almost every case. This justifies us in ascribing almost all of them to Common Uralic as well as IndoEuropean.

PRONOMINAL STEMS (only the Finnish forms are shown)

Singular and plural:
1st pers: minae
2nd pers: sinae (s- from t- before -i-)
3rd pers: haen (h- from s-, not attested in Sam.)
tae-, tuo (attested in Sam.)
nae-
e- or i-
Relative: yo- (not attested in Sam.)
Interrogative: ken

4. NOTES on Collinder's Indo-uralisches Sprachgut

**[(The above list is incomplete.)]

NOUNS, VERBS and PARTICLES (Finnish forms only, except as otherwise indicated. These words are allowed because they have cognates in Samoyedic as well as in several FU languages.):

asu- "to live or establish oneself": IE *es-
hur- (Hungarian) "a chord": IE *ghor-
kaappaa- "to grip": IE *kap-
keri- "rind": IE *(s)kor-
-ki "and" (enclitic suffix): IE *-kwe
kum (Votyak) "man": IE *ghom-
kunne (kuntee-) "to grip": IE *ghend-
kuole- "to die": IE *gwel-
kuras "knife": IE *(s)ker-
kaely "sister-in-law": IE *galwo-
lante- "lowlying field": IE *land
mon- (Hungarian) "to speak": IE *mon-
moske- (Estonian) "to wash": IE *mosg-
mur-t- "to break": IE *mer-
myy- "to trade": IE *moi-n-
nato "sister in law": IE *nat- or *gnoo- (init. laryngeal?)
ne (Hungarian) "not": IE *ne
nime- "name": IE *nom-
oshko (Cheremis) "poplar": IE *osko- "ash"
palyo- "much": IE *polw-
pata "pot": IE *boud-
pelkae- "to fear": IE *pel-
perae "far behind": IE *para
piiri "around": IE *peri
por, perk- (Vogul) "heart, stomach, soul": IE *perkw-
puno- "to wind or spin": IE *(s)pin-
salava "willow tree": IE *salik-
suone- "tendon": IE *senw-
tuo "bring": IE *doo-
vete- "water": IE *wedor
vetae "to lead": IE *wed-
viha "hate, poison": IE *wis-
vuote- "year": IE *wet

**[(Note that Collinder did not include the common pronominal roots in this list, though they are undeniably eligible.)]

Page 72: Words with a Uralic Affricate:

Collinder takes note of the fact that in both IE and Uralic there are few words which began with an affricate, i.e., a stop consonant which is immediately followed by a spirant or aspirate. Nevertheless, he was able to identify four FU words, three of them attested also in Samoyedic and hence attributable to Uralic, which are similar in sound and meaning to words in IE, as follows:

csuen- (Hungarian) "to diminish": IE *ghwdhen-
soro "to drip or flow": IE *ghwdher-
chares (Lappish) "coarse, stiff, hard": IE *ster-
tarkea "to seek or lust for", (not in Sam.): IE *sterg-

COLLINDER'S CONCLUSIONS

Collinder acknowledged the fact that since only some 30 or 40 word agreements (excluding pronouns) between Uralic and IE have been found, it was not possible to develop a comprehensive system of sound correspondences between the two groups, particularly since the laws of vocalic correspondence within Uralic itself were still unknown. Nevertheless, he ventured to make some vocalic comparisons between Uralic and IndoEuropean in regard to several of the listed words.

In regard to the consonants, such IE consonants as l, r, m, n, s, w(u) are well represented in the listed words. He quotes Wichmann in venturing to propose the following partial list of labial correspondences:

Votiak-Ziryene	p	Hung.	p	*p
" "	p	"	f	*ph
" "	b	"	b	*b
" "	b	"	f	*bh

Thus the apparent simplicity of the consonantal systems in the individual modern FU languages is misleading since it masks an earlier consonantal complexity which must be assumed to have once existed in Common FU in order to account for the variegated reflexes displayed by the modern languages. (The starred consonants at the right are those proposed for Common FU).

Wichmann and other investigators have proposed similarly variegated series for velars and dentals. **[(1982 NOTE: Thus it appears that Common FU must once have had a complicated conso-

nantal system which was rather similar to that which has been reconstructed for IE.)]

*The question arises as to whether Collinder's list of common words is due to borrowing between Uralic and IE or whether it represents a vestigial retention from an earlier IndoUralic "genetic" unity. That it was not due to borrowing is suggested by the fact that the meanings represented by these words are not concentrated in only one or two realms of human activity and concern, as is normally the case when borrowing occurs. Instead, the meanings are about equally distributed among a dozen different categories. In fact, two of the instances are enclitics (IE -kwe and ne), which are very unlikely to have been borrowed. **[(1982 NOTE: Additional evidence against borrowing between Common Uralic and Common IE consists in the fact that a number of the above words exist also in other "Nostratic" languages -- see Collinder in 1.5)]*

When we add to this the great number of common derivational suffixes and morphological elements, and especially the large number of common pronominal roots and affixes, none of which is likely to have been borrowed, it becomes clear that some cause other than borrowing for these agreements must be proposed.

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to INDO-URALISCHES SPRACHGUT

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END OF NOTES on INDO-URALISCHES SPRACHGUT

**[(1982 COMMENT: On recently rereading the Indo-uralisches Sprachgut I have come to the conclusion that Collinder's repeated disclaimers (to the effect that he has failed to prove the IndoUralic hypothesis) are quite unjustified. His only objection has been that "there are very few common words", but he himself has recently shown that when widely separated present day spoken IndoEuropean languages are compared on a pair-wise basis, without reference to extraneous or extinct languages, the number of common words turns out to be only a little more than has been counted for the comparison between Common Uralic and Common IndoEuropean (again on a pair-wise basis, it should be noted), despite the greater antiquity of the latter connection; to me, this indicates quite clearly that the apparent paucity of common words is not a valid objection against the IndoUralic hypothesis.

More significant is the highly specific nature of the numerous morphological agreements. In addition to about a dozen deverbal and/or denominal formants which agree in form and meaning, we have three verbal present stems, two verbal preterite stems, an oblique case stem in -n-, a casus rectus of pronouns in -t (-d in IE), an oblique plural of nouns in -i-, a nominative plural of pronouns in -i, an ablative in -ta (-d in IE), and an accusative in -m. This accusative in -m was actual-

ly a definite-accusative (seldom being used except when the object was definite), and I have shown that this was the basis for the origin of gender in IndoEuropean, which otherwise cannot be explained.

In addition, I have shown that the number of common pronominal forms, bound and unbound, shared by Uralic with both IndoEuropean and Altaic is statistically significant, and highly so. I have also shown that these resemblances cannot be explained by borrowing.

These facts, considered together as they certainly ought to be, are overwhelming proof of the IndoUralic hypothesis. Anyone who doubts this ought to be required to present evidence and arguments on the other side, yet no one has managed to do this with any cogency. The situation, after all, is dichotomous: Either Common IndoEuropean and Common Uralic separately evolved from a single earlier language ancestral to them both, or they did not. If they did, then we can easily account for the numerous and highly specific morphological and pronominal agreements. But if they did not, then some other explanation for these agreements must be proposed.

The proposition that the agreements are without significance, presumably having developed independently, is destroyed when we recognize the fact that a like number of similarly significant agreements is not observed when we attempt to compare IndoEuropean (or Uralic or Altaic) with languages outside the "Nostratic" group, such as Chinese, Malay, Bantu, aboriginal Australian languages or American Indian languages (except EskimoAleut, of course). And even when we restrict the comparisons to the "Nostratic" languages, it is clear that the similarity between IndoEuropean and Uralic outweighs that which is found in any of the other comparative arrangements.

Also, the proposition that the agreements are due to borrowing between Common Uralic and Common IndoEuropean is likewise insupportable. On the contrary, all the available evidence indicates that the speakers of the two languages must have lived in separate geographical areas under conditions which precluded borrowing. Besides, the noted morphological and (especially) pronominal features involved in these agreements are precisely the kinds of linguistic features which are observed to be least subject to borrowing.

Further, the fact that the similarities between Uralic and IndoEuropean are less abundant than between coeval branches of IndoEuropean (or of Uralic) is no more significant than the fact that English is less similar to Russian than to German. The IndoUralic unity is merely one step farther back into the past.

Under the circumstances, all cavils to the effect that "no proof has ever been offered", or that the IndoUralic hypothesis is "unproved" or "not quite proved", degenerate to mere obscurantism, which is quite unworthy of serious scholars. I use the word "obscurantism" advisedly, for it is most appropriate. It is now more than a century since Nicolai Anderson wrote his seminal work, and more than 40 years since Collinder wrote his Indo-uralisches Sprachgut, yet scholars have been quibbling ever since. -- JCKerns)]

NOTES on EXCERPT from DIE URHEIMAT der INDOGERMANEN, pages 224-225, WISSENSCHAFTLICHE BUCHGESELLSCHAFT, Darmstadt, 1968, by S. E. Mann, translated into German by Renate Saurer.

Title: Die Urheimat der "IndoEuropaeer" (originally The Cradle of the "Indo-Europeans").

***[(1982 NOTE: This chapter is based on material sent to me by Prof. Mann in return for sending him a copy of my pamphlet of 1966-1967. It is presented here in the form of notes to provide a representative example of what I consider to be a serious and competent attempt at IndoEuropean linguistic paleontology. As elsewhere in this book, long vowels are indicated by simply doubling them. -- JCKerns)]*

Through a process of elimination, Mann proposed to arrive at a plausible suggestion for the location of the region where the as-yet-undivided IndoEuropean language was spoken. The process included determining the geographical limits of some of the animals and plants which are assumed to have been known by the undivided IE people.

**bhebhr-us, -os -- "beaver": This occurs in Old Indic with the meaning "red-brown". It does not occur in Greek, Armenian or Albanian, but it does occur in an old Dacian word in Rumanian as 'breb'. The Avestan 'bavra', "beaver", appears to be the same word as Ossetic 'babur', "yellow", and Persian 'baebaer', "lion". The word is well represented in Germanic and Baltic languages, but not in present-day Celtic languages; it does occur, however, in Old Welsh. From the above traces, it appears that in former times beavers were restricted to Northern and Eastern Europe and the Western and Northern parts of Asia, that they were named for their reddish-brown color, and that they lived in forested lands with small and gently flowing streams. Until recently, they were still to be found in the Rhone valley, and in Pleistocene times they occurred as far south as Rome. Today they still occur in Poland and Lithuania. They recently became extinct in Lappland, but in the neighborhood of Arendal, Norway, there still exist three large colonies.*

**waaiweraa, wiweraa -- "squirrel": The first of these occurs in Baltic, the second in Celtic and Latin. Neither occurs in Greek, Albanian, Indic, Iranian or Armenian. Despite the long vowel in the Baltic form, the name seems to go back to the root *wei- "to wind or twist". Although the squirrel is distributed through most of the world, including Sweden and Southern Norway and in Northern Asia, it is unknown in Southern Italy, the Caucasus and the Crimea.*

*muus -- "mouse": Occurs in all but Celtic and Baltic, though it also is used to designate the rat in Albanian, Armenian and Persian.

*wlqos -- "wolf": Occurs in all but Celtic and Armenian, in both of which the words for "wolf" come from *wailos.

*alanis, *elanis -- "deer": Occurs in Celtic, Germanic, Baltic, Slavic and Armenian. Deer occur throughout Europe except in Russia, where it occurs only in the Crimea and the Caucasus. In Scandinavia and in Mediterranean lands it is not as abundant as in the woods of Central and Eastern Europe.

*eg'his -- "hedgehog": This form of the word is found in Baltic, Slavic and Old Phrygian. Derivative forms occur in Greek, Germanic and Ossetian. The hedgehog is found throughout most of Europe, Asia Minor, Syria and the Caucasus.

*ek'wos -- "horse": This occurs in Latin, Germanic, Celtic, Baltic, and in both branches of IndoIranian. The aberrant form in Greek (hippos) suggests a loan from some alien source; Mann suggests Asia Minor for the initial h-. Mann rejects the idea that *ek'wos ever had the meaning "swift"; he derives words for "swift" from roots like *elnghus or *elnghwos, *twrto's, and *tnghus. In regard to the question as to whether the IndoEuropeans had domesticated the horse, Mann cites words derived from *sedhlom and *sedhlaarios (saddle and saddler, in Germanic and Slavic), from *bhrunyos and *bhrunyaa (horse harness, in Germanic, Slavic and Baltic, related to Irish "bruinne", breast or brow), and words derived from *yaay- in Baltic and Indic. Mann also cites a few other roots occurring in Baltic, Germanic and Greek, such as *gwina, *toksos and *toksya, but these seem to have to do with hunting or with herding in general, not referring specifically to horses. To me, the implication of horse domestication is unconvincing because the meanings of the roots are either too vague, or else the roots are too restricted in their occurrence; we see the local and late assignment of new meanings to old words.

*luk'sis, *luk'snis, *luk'snos -- "lynx": Occurs in Baltic, Germanic, Greek and Armenian. Lives in Northern Eurasia though now extinct in and around Central Europe.

*lak'asos -- "salmon": This occurs in Germanic, Baltic, Slavic, Tocharian and Ossetian, but in Tocharian with the general meaning "fish". Russian 'lox' may have been borrowed from a neighboring IE language. In Eurasia the salmon is restricted to regions north of the 42nd parallel and does not occur in rivers which flow into the Mediterranean and the Black Sea. Mann then went on to consider words for "sea", stating that words derived from *marya do not occur east of the Caucasus and Asia Minor with this meaning, and that in IndoIranian the meaning is "empty or barren field". Another word for "sea" (and ancestral to it) he derived from *saiwos, found in Germanic, Celtic and Latin, originally meaning "restless".

*treskos, -aa; *trskos, -aa -- "codfish". Said by Mann to be wide spread.

*k'ormoo or k'armoo -- "weasel" or "ermine": Found only in Germanic and Baltic.

*ghansis -- "goose": Believed by Mann to be onomatopoeic. Occurs in Germanic, Baltic, Slavic, Latin, Greek, Celtic and Indic. The birds belong mainly to Northern Eurasia, wintering in the Mediterranean regions.

*anatis -- "duck": Occurs in Latin, Baltic, Slavic and Germanic.

*udros -- "otter": Occurs in Baltic, Slavic, Germanic and Ossetian with this meaning. The same word occurs in Greek with the meaning "water serpent" (hydra).

*mewos, -aa -- "mew" or "seagull": Occurs in Germanic and Baltic. In Irish it has the meaning "hen".

*trusdos, *tursdos -- "thrush, throstle": Occurs in Latin, Old Irish, Germanic and Armenian. Five kinds of these birds are found in Europe and North Asia.

*teterwos, -aa; *tetrwos, -aa -- "fowl, game bird": Occurs in Germanic, Baltic, Slavic, Iranian, Armenian and Greek.

*qwrmis -- "worm": Occurs almost universally.

*ghelwos, -aa; *g'helwos, -aa -- "turtle": According to Mann, this was one of the IE words for "yellow", apparently from the dominating color of the turtles in the Balkans. Cognates of this word with the meaning "yellow" occur in Germanic, Baltic, Latin, Ligurian. Apparently the "yellow" turtle is not native to Central and Northern Europe.

**wospaa, *medhu, *melit -- "wasp, mead, honey": There appears to have been no common word for "bee", yet since the words for "mead" and "honey" are almost universal, bees must have existed in the territory of the undivided speakers of IE.*

**bherg'os, -aa -- "birch": Cognates occur in Germanic, Baltic, Slavic and IndoIranian, though various varieties of birch were thus designated. The name meant "clear" or "bright". Birch trees occur throughout most of temperate Eurasia.*

**salikis, *wiitis, -yaa -- "willow": Cognates of the former occur in Celtic, Latin, Greek and Germanic; cognates of the latter occur in the same plus Baltic, Slavic, IndoIranian, Armenian and Albanian. The willow is a Northern tree found throughout most of Eurasia.*

**apsaa, epsnoo -- "aspen": Cognates occur in Celtic, Germanic, Baltic and Slavic. Found throughout Eurasia and North Africa.*

**oosis, also with formant -k- or -n- -- "ash": Occurs without a formant in Baltic, with -k- in Germanic, Armenian, Albanian and Greek, with -n- in Slavic, Celtic and Latin. Seems not to occur in IndoIranian. Has the meaning "beech" in Albanian and Greek. Found throughout most of Eurasia.*

**eiwaa -- "yew": Has this meaning in Germanic and Celtic, but other meanings in Baltic and Slavic.*

**meli, melyom, melyaa -- "millet", that which has been "milled". Occurs in Latin, Celtic, Slavic, Greek and Albanian.*

**modharos, -aa -- a dye plant. Occurs in Germanic, Celtic, Baltic, Slavic, IndoIranian and Albanian.*

**weroik'os, -aa; *wroik'os, -aa -- "heather". Has this meaning in Celtic, Slavic and Greek, and other meanings in Lithuanian, Latin and Albanian.*

**meelis -- another dye plant. Derivatives denote various colors in most Post-IE languages.*

**musos, *mustos, *muskos -- "moss". Occurs with this meaning in Germanic and Slavic, and with other meanings in Baltic, Celtic, Armenian, Indic and Latin.*

5. NOTES re S E Mann

**wiskis, -os -- "mistletoe, twig, or berry": Occurs in Slavic, Latin, Greek, Albanian and Celtic. Mann goes on to list eleven words of non-IE origin for various plants, borrowed into several IE languages, as follows:*

**bhak'os -- "bean": In Greek and Albanian.*
**bhabhos, -aa -- "fodder bean, vetch": In Latin and Slavic.*
**maakos, *maakoo -- "poppy": In Greek, Slavic, Germanic and Armenian.*
**wikis, *wiikis, *wiikyaa -- "legume": In Latin, Baltic and Slavic.*
**lakhano- -- "greens, vegetables": In Greek and Albanian.*
**linom, *liinom -- "flax": In most European languages, originally only in Latin and Greek.*
**kanaphis, *kanabis -- "hemp": In Greek, Albanian, Germanic and Slavic.*
**k'ik'er -- "chickpea": In Latin, Armenian and Albanian.*
**erogwos -- "pea": In Greek and Latin.*
**pisos -- "pea": In Greek and Latin.*
**malaghwaa -- "mallow": In Latin, Greek and Albanian.*

MANN'S RECAPITULATION:

Mann says that an examination of the way of life of the IE people enables us to gain a greater insight into the problem of locating the IE homeland.

*There are no common words for tropical and subtropical plants and animals; however, there are also no common words for several West European creatures. The fact that there is no common word for larch, a widely spread European tree, and that common words for pine and fir are hard to establish, shows that not too much significance should be put on negative indications. There are no common words for grass or stone, although a common word for milking suggests that the IE people ought to have been acquainted with grass. The fact that grass was mowed as fodder for animals is suggested by *k'oinom, -aa -- "hay", which occurs in Greek, Baltic and Slavic. Several common words for items of cultural significance are:*

**gwoows -- "cow"*
**owis -- "sheep"*
**suus -- "swine"*
**aig'is, -os -- "goat"*
**k'unes -- "dogs"*
**wog'hoi -- "wagons"*

*derwos -- "wood"
*rothoi -- "wheels"
*kwekwoloi -- "wheel rims"
*ak'sis -- "axle"
*tulis, -os -- "linch pin" (achsennagel)
*yugom -- "yoke"
*ruugaa -- "road"
*aroo -- "I plow"
*seemoo, -en- -- "seed"
*seeyoo -- "I seed"
*kerpoo -- "I harvest"

According to Mann, the special meanings associated with these words, and the fact that each of them has reflexes in several branches of IE, show that the undivided IE people lived in a country with a temperate climate, having many meadows and arable fields, with an ample supply of wood and stone for making highly effective tools.

Mann goes on to assert that no one will deny that the undivided IE people lived in a high state of culture, that they must have had many tools appropriate for agricultural life, including plowing, sowing and harvest, felling trees, and using wagons made of wood. An undoubted word for a metal (*ayos, esos -- "bronze") is only marginally attested (in Latin, Germanic and IndoIranian). In regard to tools and their use, he cites the following:

*aratrom, *aradhlom -- "plow"
*mateyaa -- "pick" or "mattock"
*ak'moo, -en- -- "stone anvil"
*sekuuris, -aa -- "cleaver"
*ksuro's, *ksurya -- "razor"
*g'ombhos -- "nail"
*ak'is -- "sharp instrument"
*ghebhalos -- "spit" or "pitchfork"
*gwernowos -- "churn"
*peilos, -aa -- "rasp"
*teredhlom -- "auger, awl"
*sedhlom -- "saddle"
*srpos, -aa -- "sickle"
*syuudhlom -- "awl"
*needhlom -- "needle"

Despite these examples, Mann concludes that the use of metals did not begin until the differentiation of the IE languages had already begun. I agree with him on this point.

5. NOTES re S E Mann

Mann included a map which indicates the Baltic area -- Eastern Poland and Lithuania, and adjacent parts of European Russia -- as the region where he thought the undivided IE people originated.

MY OWN REACTION

to Mann's effort is one of admiration for the thoroughness, competence and integrity of his research and presentation. However, in any investigation of this kind there is a difficulty which I believe to be insuperable. It consists in the fact that there were many opportunities for the names of flora and fauna, of tools and weapons, and of skills and institutions, to cross territorial and dialectal lines throughout the long succession of post-expansion eras. One example of this sort of thing (much later) in historical times is the adoption from contemporary Latin of skriiv- ("to write", where the v represents the voiced bilabial spirant in this case) in most of the early historically known Germanic languages.

*Another example in prehistoric times (but Post-IE) is the Satem root *pis- which seems to be derived from an IE root meaning to mark or paint; in Iranian and Slavic, however, it has the meaning "to write". This is remarkable because at first glance the sharing of this meaning appears to date from a fairly late Post-IE period (ca. 2500 BC ?) when the speakers of Pre-Indo-Iranian and Pre-Slavic were still in close enough proximity to share a number of innovations, including semantic innovations, transmitted across dialectal boundaries. But since it seems unlikely that either of these groups of people knew any method of writing at this early date, the assignment of the meaning "to write" to what was historically the same word must have occurred independently in the two branches and at much later dates.*

A similar example in English concerns the verb "to write" and its many cognates in the other historically known Germanic languages. In English, this is its only meaning; but in the other Germanic languages it also has the meaning "to tear or scratch", apparently in reference to Runic writing on wood in the early Post-Germanic period. And its more remote cognates in Greek and Sanskrit have only the latter meaning.

Examples such as these point out the difficulty of drawing conclusions with much confidence from this sort of investigation. It is always the case that what is known concerning the phonetic, morphologic and semantic development of interesting *words is far outweighed by what is unknown, particularly when the words seem to have originated in prehistoric times. I suspect that the secondary assignment of new meanings to old words was a common occurrence in post-expansion times, and that the borrowing of cultural ideas and practices and their corresponding verbal expressions, and even occasional morphologic features, across post-expansion interdialectal boundaries was considerably more prevalent than is ordinarily realized. This is particularly credible when we consider the peripatetic habits of those Post-IE tribes whose histories we happen to know something about.*

Even a strict application of Neogrammarian phonemic analysis can sometimes yield results whose implications are illusory. For example, when the Greeks in the Classical period became acquainted with the Persian Empire they borrowed the national name of the Maadaa as "Meedoi", substituting the eta for the long alpha. At first glance this seems to imply that the vowel shift of long alpha to eta must not yet have occurred in Attic-Ionic. However, there are other considerations which lead to the conclusion that the shift occurred considerably earlier, long before the Greeks had any knowledge of the Medes and Persians; hence it appears that the word was first adopted as "Meedoi" in Eastern Ionic dialects for the simple reason that the long alpha no longer existed in these dialects and that the eta was the nearest equivalent. Since original eta had remained nearly unchanged in almost all the Greek dialects, there was no impediment to the secondary adoption of the Eastern Ionic form by most of the other dialects, including Attic where the vowel shift had been partly reversed by an incomplete restoration of the long alpha -- of course, this implies that the original eta in Attic had a more closed articulation than the shifted long alpha at the time of the restoration, which kept it apart until after the reversal had been completed.

*Another example of this kind can be seen when we consider Sanskrit "cakra-", cognate to English "wheel" and having the same meaning. The Sanskrit form seems to have evolved (by secondary palatization and vowel changes) from an early Satem *keklo-, where both the k's seem at first sight to have resulted from a delabialization of the labio-velars contained in an assumed earlier *kwekwlo-, as suggested by the reflexes in Centum languages. At first glance this seems to imply that vehicles with wheels were an original possession of the undivided IE people, predating the inception of the Pre-Satem delabializations. It suggests that the use of wheels was either very early or else that the onset of Satemism was late; this second alternative is quite credible since the distribution of Satemism among the* *historically known Post-IE languages shows unmistakably that it was a post-expansion development.*

*Nevertheless, there remains the possibility that the adoption of wheels and the coining of the word *kwekwlo- were Post-IE and Post-Satem developments in some Centum-speaking region and that they were later spread to other post-expansion dialect areas, including early Satem-speaking regions. In Satem languages the word would naturally appear as *keklo-, precisely as if it were primordial, because labiovelars no longer existed in these dialects and the non-labial velar k was the closest equivalent. Thus the proposition that the use of wheels and the corresponding word *kwekwlo- were primordial is not compelling since the fundamental requirement of reductio ad absurdum is not fulfilled -- the proposition, while certainly plausible, is not the ONLY plausible suggestion that can be made to explain the phenomena.*

*Still another example can be seen in the Germanic root "riik-", meaning some sort of tribal chief or king. A comparison among the reflexes in Latin, Sanskrit and Celtic leads to the suggestion that the form in IE was originally *reeg- since this is the ONLY reconstruction that would yield the observed reflexes in all three of these languages; in particular, the Celtic form "riig-" regularly reflects the shift of original ee to Celtic ii. The Germanic form, however, is irregular since this shift did not occur in Germanic. Therefore we conclude that the Germanic word must have been borrowed from Celtic since this is the ONLY suggestion that seems to be plausible (thus it meets the fundamental requirement of reductio ad absurdum).*

At first glance this seems to indicate that the borrowing of the Celtic word must have antedated the general Germanic devoicing of original voiced stops, suggesting a late date for the Common Germanic consonantal shifts (first millennium BC ?) since it is usually assumed that the Celtic expansions did not begin until the Hallstadt Iron Age (8th century BC ?).

But there are two reasons for doubting the lateness of the Germanic consonantal shifts on the basis of this particular criterion. One reason is that there appears to be some justification to suppose that the Celtic expansions actually began during the European Bronze Age (Urnfield Culture), which would take us back to something like 1500 BC as a possible date for the beginning of Celtic influence upon Germanic. The other reason is that even if the Germanic shifts had already occurred the observed Germanic form would still have resulted, for the k in "riik-" would still have been the only available intervocalic velar stop and would still have been the nearest equivalent to Celtic g. It is significant that the simple voiced velar in Germanic was a spirant at this stage, and it remained so in all positions until Post-Germanic times. It was certainly so in Old English even in initial position, as we know from the poetic alliterations, and it is still a spirant there in Dutch though now unvoiced. Thus it appears that the assumption of a late date for the Germanic consonantal shifts, by this criterion, is not compelling since it is not the ONLY plausible explanation for the phenomenon in question. (Thus it fails to meet the fundamental requirement of reductio ad absurdum.)

I have gone to considerable length with the foregoing examples to illustrate some of the many pitfalls that lie in wait for those who would engage in the kind of investigation which Mann undertook; accordingly, we are compelled to be tentative in our conclusions. Nevertheless, I agree with Mann in his rejection of an Asiatic homeland for the undivided IndoEuropeans since the lack of common words for uniquely Asiatic species is apparently complete, making it unlikely that it could have been due to sporadic losses. Thus I conclude that the homeland must have been somewhere in Europe.

On the other hand, because of the demonstrated ease of post-expansion interdialectal borrowing, together with the readiness to assign new meanings to old words, and because of the frequent difficulty of providing unambiguous explanations for the history of individual words, I cannot uncritically accept Mann's designation of the Eastern Baltic region as the pre-expansion homeland of the IE people. For the reasons I have stated, I suspect that this and all other exercises in Indo-European linguistic paleontology will inevitably give a false picture of the culture and concomitant geographical situation of the IndoEuropeans in their pre-expansion phase.

The picture is false in that it makes the technological status of the IndoEuropeans seem rather higher than it actually must have been. And for the same reasons, the apparently indicated homeland may well be far removed from the true pre-expansion focus. In short, the picture provided by linguistic paleontology pertains rather to a post-expansion situation. Furthermore, the picture is incomplete, and grossly biased, in that it fails to take the Anatolian branch into consideration.

In this connection, it is significant that the earliest archeologically discernible Neolithic culture in the East Baltic region, the Funnel Beaker Culture, postdates the Linear Pottery Culture of Central Europe and adjacent regions and indeed appears to have been one of its several successor cultures. The archeology of the Funnel Beaker Culture suggests that its people were strongly expansive at a rather early period. If we (very *tentatively) assume that the Eastern members of these people spoke an early form of Baltic, we would have a plausible explanation for the occurrence of Baltic place names as far east as Moscow, a powerful Baltic influence on the language of the early Slavs to the south, and considerable influence on the early Germans to the west.*

6. HORSE DOMESTICATION

The question as to whether the undivided IndoEuropeans were in possession of domesticated horses is one which has constantly vitiated the understanding of scholars concerning the nature of early IndoEuropean society. The assumption in favor of the affirmative seems to be universal, for I do not know of any scholar who has ever expressed any serious doubt that horse domestication was a major feature of IndoEuropean life from the very beginning. For example, the assumption apparently is unquestioned in the minds of such linguistic scholars as W. P. Lehmann, Paul Friedrich and others, for their unrestrained admiration for the performance of Marija Gimbutas in her 1963 and 1970 articles would make no sense without the prior and unreserved acceptance of this proposition.

Until I started to write my 1966-67 pamphlet I too had shared this assumption, for in my ignorance I thought it entirely "reasonable". After all, we have historical knowledge of such "Indo-European" groups as Scythians and Sarmatians and other IndoIranian peoples, together with Greeks, Latins, Celts, Germans and Slavs, all having domesticated horses. And the profound impression made by such equestrians as the Huns, Goths and Mongols made it seem natural to assume that the wide geographical spread of the IndoEuropean linguistic family would have been impossible without the initial possession of such an egregious military advantage.

My opinion on this topic was reinforced some 30 years ago when I read V. Gordon Childe's "The Aryans, A Study of Indo-European Origins", (1926). Childe wrote this work as the first publication in English, since the turn of the Century, to summarize the findings of linguistics and archeology as they might be brought to bear, at that time, on the question of locating the homeland of the undivided speakers of IndoEuropean. It followed shortly after the first publication of his "The Dawn of European Civilization", whose sixth and final edition appeared in 1957.

Two peculiarities of Childe's terminology should be noted. One is the use of "Aryans" in the general sense of "IndoEuropean"; the other is the use of "Danubian I" to designate the archeologically identified culture in Central Europe which is usually called "Linear Pottery". In my pamphlet I adopted Childe's term for the latter, but in this book I conform to the usual practice though I do not personally prefer it.

"The Aryans" was written at a fairly early point in Childe's career, yet it already shows signs of the careful investigation and balanced judgment for which he was justly famed

and is still admired. It is true that there is now a justified tendency to disparage his assumption concerning the seminal importance of cultural diffusion from the East Mediterranean in the development of prehistoric European cultures, but a careful reader of his later works will observe that he was beginning to depart from that assumption despite the fact that the tree ring correction of radio carbon dating was not yet available in his lifetime.

When I read "The Aryans" I was convinced by it, for in my ignorance I accepted Childe's assumption concerning the crucial importance of horse domestication to the undivided IndoEuropeans, and thence I concurred in his rejection of the Linear Pottery Culture as a candidate for identification with the first IndoEuropeans in Central Europe. By default of all other suggestions, I agreed with his selection of either the Nordic people of Northern Europe or other Europoid people in Southern Russia. It was only some 16 years ago, while I was writing my pamphlet, that I began to see the fallacies in his analysis.

Unfortunately, Childe's book, though argued with meticulous care, is flawed by unproved assumptions which are necessary to his argument yet which in some cases can be seen to be erroneous because they lead to conclusions which are at variance with observational facts.

Childe made the following assumptions, which I consider wrong:

a. He assumed that there was a reciprocal causal relationship between the structure of a language and the achievements of the people who speak it, stating "...to inherit an exceptionally delicate linguistic structure gives a people a vantage point on the path of progress." Thus Childe anticipated the Whorf hypothesis, which I do not accept. On the contrary, I hold that a given language is shaped by time and the accidents of history which overtake the people who use it, and that the achievements and sufferings of a people have little to do with the language they use except insofar as the identity of their language may have to do with the social and political prejudices of the times. Besides, there is no reason to believe that the "delicacy" or expressiveness of Common IndoEuropean was actually exceptional; specialists in various non-IndoEuropean languages are quite capable of maintaining, successfully, that their favorite languages are second to none in their expressive resources. Eskimo, for example, has inflectional resources which put Greek and Sanskrit to shame.

b. Childe shared the racism of his time, which impelled him to regard the spectacular activities of peoples historically identified as speaking "Indo-European" languages as due to an intrinsic superiority of mental and physical powers of the individuals comprising these tribes. He was particularly enamored of the idea that these activities were due mainly to intrusion by Nordic and/or other non-Mediterranean Europoid peoples ranging between Northern Europe and Southern Russia.

My comment on such a notion is that the identification of people of Nordic descent with the speakers of early historically known Post-IE languages has never been clear. Quasi-Nordic physical types seem to have been only a minority in the Southern parts of Post-IE speaking territory, and even in the Northern parts there seems to be little to show that any Nordic population could rationally be identified with the original speakers of IndoEuropean since the fundamental requirement of reductio ad absurdum is unfulfilled; plausible alternative suggestions can be made.

*c. Childe shared the almost universal conviction that the undivided IndoEuropeans had domesticated some variety of horse, either the native European forest horse or a smaller and swifter horse of Western Asia. He based this notion on the wide-spread occurrence of reflexes of a word for "horse" reconstructed as *ekwos, where the k is assumed to have been palatal on the evidence of the reflexes in Lithuanian and IndoIranian. He also assumed that the primordial meaning of *ekwos was "swift", apparently deriving it from an adjective *ooku's, where I show a double-o to indicate that the vowel in the first syllable was long; again, the k is assumed to have been palatal on the basis of reflexes in IndoIranian. Also, the accent is assumed to have occurred on the second syllable on the evidence of the form in Greek. It was on the basis of such assumptions as these that Childe concluded that the Linear Pottery Culture is to be rejected for serious consideration as that of the undivided IndoEuropeans, for there was (and still is) no discernible evidence that the Linear Pottery people had ever domesticated any horse-like creature prior to the supersession of their culture by various successor cultures.*

Of course, the lack of apparent evidence for horse domestication by the Linear Pottery people is not altogether convincing since absence of evidence is not evidence of absence. We must remember that these people lived in a pre-metallic age; hence little was preserved in their loess soil that was not made of pottery, stone or bone, and it is possible that the items associated with primitive horse domestication were made of such organic materials as would not have been preserved, particularly *since the soil was subject to considerable erosion. Nevertheless, I am willing to accept the lack of evidence at face value and to assume, like everyone else, that the Linear Pottery people had not domesticated any horse-like creature at the time of their expansion from the Upper Danube.*

*In any case, the issue of horse domestication is irrelevant to a possible identification of the Linear Pottery people as the first discernible IndoEuropeans in their region, for the idea that the undivided IndoEuropeans had domesticated some variety of horse is quite unsupported by any evidence worthy of the name -- it has never been anything more than an assumption. Scholars who comment on this topic generally accept the idea blindly and without reservations, or they say that the domestication was "probable", but they never present any reasons, based on actual evidence, to show why anyone should accept the notion. So far as I can tell, the idea seems to be suggested to them by the wide occurrence of reflexes of *ekwos, but this no more implies domestication than does the occurrence of widely spread reflexes of names for other creatures, many of which are assumed to have been wild. There can be no question that the Linear Pottery people knew some kind of horse since horse bones, though sparsely represented, have been found in a majority of Linear Pottery sites. It seems not unreasonable, therefore, to assume that they had a name for it, or perhaps several names. Whether *ekwos was one of those names is something we cannot determine, but it may well have been.*

*Childe's derivation of *ekwos from (apparently) *ooku's was the major point in his rejection of the Linear Pottery people as the first identifiable IndoEuropeans. On the basis that *ekwos meant "swift", he assumed that this ruled out the Linear Pottery people since they presumably would not have been aware of the relatively small and swift Asiatic species.*

*But a derivation of *ekwos from *ooku's is clearly false since the long vowel in the first syllable of *ooku's (presumably due to a vowel-lengthening laryngeal) is incompatible with the short vowel in the corresponding syllable of *ekwos; hence an appeal to ablaut will not suffice to reconcile them. Neither would an appeal to vrddhi, for this would imply a change from short vowel to long, not from long to short. Furthermore, the accent is wrong, for if *ekwos were really of adjectival origin we should expect its accent to occur on the second syllable, yet all the historically discernible reflexes show it on the first.*

*Thus we have to conclude that *ekwos and *ooku's are no more historically connected than are "God" and "good" in Germanic languages; the resemblance is fortuitous and logically indefensible. This means that we really have no information con-*

*cerning the primordial derivation of *ekwos; hence any employment of it in this investigation is meaningless.*

*And even if it were true that *ekwos actually did mean "swift", this still need not imply a contrast between two types of horses; it could just as well (and more likely) imply a contrast between a local wild horse and certain slower and presumably domesticated animals with which the IndoEuropeans happened to be familiar, such as swine or sheep -- the fundamental requirement of reductio ad absurdum is unfulfilled. But as I have said, the idea that *ekwos once had this meaning is merely an assumption, quite lacking in evidential support.*

There are two other considerations, both mentioned earlier, which may have been conducive toward the common assumption that the undivided IndoEuropeans had domesticated horses. One of them is the historical example of such equestrians as the Goths, Huns and Mongols, whose spectacular military achievements make it seem natural to assume that the IndoEuropeans could not have made their impressive expansions without similarly exploiting the domestication of horses from the very beginning. But the speciousness of this line of "reasoning" becomes apparent when we consider the examples of such widely spread linguistic families (and concomitant tribal groups) as, for example, the Athapascan, Algonquian, UtoAztecan, MayaSoke and Aymara in the New World, and the AustroAsiatic in Southeast Asia and the Dravidian in the Indian subcontinent, for at least some of these expansions (possibly all) were accomplished without the help of domesticated horses. Consequently, there is no reason to assume that the domestication of horses was a necessary condition for the expansion of IndoEuropeans, and on the basis of Ockham's Razor we ought not assume it.

The other consideration involves such historically known peoples as the IndoAryans, Iranians, Slavs, Germans, Celts, Greeks and Latins, all of whom are known to have spoken "Indo-European" languages, and all of whom were in possession of domesticated horses at their first emergence into history. It is tempting to extrapolate backwards into time and to assume that horse domestication was a feature of the "ancestral" undivided IndoEuropeans as well. But this assumption has a fatal flaw, for the inescapable anachronism is unacceptable; there is no reason whatsoever to assume that the technological and social status of these peoples had remained unchanged. The above peoples were not IndoEuropeans but various varieties of Post-IndoEuropeans, with various mixtures and recombinations of genetic constitutions; the languages they spoke were not that of the undivided IndoEuropeans but various Post-IndoEuropean groups of dialects which are believed to have evolved from it; the *cultures that embraced them and the concerns that motivated them were not those of the undivided IndoEuropeans but various new cultures and concomitant concerns.*

This distinction is commonly ignored, but it is crucial to any rational treatment of this matter. It is significant that there was a rapid and parallel advance in technology in all the arts of life (and death), throughout the affected regions, over a period of several millennia. This advance began at least as early as the initial expansion of the Linear Pottery people in Central Europe and has continued unceasingly into modern times, irrespective of intellectual Dark Ages, and despite the archeologically observable succession of cultures in the affected regions -- the archeological record is quite clear on this point. Judging from the variegated appearance of the archeological record, I conclude that the advance has been due mainly to stimulus diffusion among the cultures in the affected territories and may well have had only a minor correlation with linguistic changes.

These are the considerations that have impelled me to reject the common assumption of horse domestication by the undivided IndoEuropeans, and hence to reject the usual exclusion of the Linear Pottery people as IndoEuropeans. So far as I can tell, I stand alone in this. Nevertheless, the fact that everyone else seems to have the same opinion as Childe is irrelevant since facts and opinions are quite different things; authorities have proved to be wrong many times, and a majority vote has no significance where facts are concerned.

The plain fact of the matter is that there really is no reason to reject the Linear Pottery people as IndoEuropeans.

7. FOUNDATIONS of NOSTRATIC

My principal purpose in writing my pamphlet of 1966-67 was to test the IndoUralic hypothesis by statistically assessing the pronominal agreements between the two undivided languages. The results were overwhelmingly in favor of the conclusion that the agreements imply a unique historical cause. The hypothesis that the agreements were due to borrowing was examined and rejected, leaving only the hypothesis of "genetic" relationship as a viable suggestion. Thus the IndoUralic hypothesis may legitimately be regarded as proved for the simple reason that there is no credible alternative.

But the "genetic" connection between IndoEuropean (IE) and Uralic is not unique; the connection between Altaic and both Uralic and IE is less obvious but no less genuine. But even this three-way grouping is not exclusive, for some of the distinguishing marks of relationship are present in other linguistic families also, mostly in or near Eurasia. Conspicuous are several pronominal forms, whether bound or unbound, such as interrogatives in k- and/or m-, a 2nd pers sing and plur in t and/or s, a 1st pers sing in a velar such as k with nasal and spirant variants, a 1st pers sing and plur (or dual) in m, and a non-nasal labial (w, p or bilabial v) in the 1st pers dual or plural, plus demonstrative elements in t, e, i, o and k. Some or all of these marks are present in an unbroken continuum from IE through Uralic and Altaic, certain "PaleoSiberian" languages, and conspicuously in EskimoAleut. Geographically, this continuum extends from Portugal through Central and Eastern Europe, Russia, Central Asia, Northeast Siberia, Alaska, and Northern Canada to Greenland (ignoring modern overseas expansions of IE).

Yet even this extended grouping is not exhaustive, for a number of investigators would also include Dravidian, certain Caucasian languages and AfroAsiatic; this further enlarged group of controversial "genetic" connections is conventionally called "Nostratic". It is discussed by Collinder in 1.5 and briefly by Prof. Comrie (24.1, p 11) in his "The Languages of the Soviet Union" (1981). Comrie's excellent book is essentially a survey so he quite properly takes a conservative view of controversial "genetic" groupings. In this discussion I will mainly ignore the languages in the Southern Group (Dravidian, the Caucasian languages and AfroAsiatic) since I know little about them and they seem a bit off the track, both geographically and linguistically. There remains the continuum ranging from Portugal through Central Asia to Greenland, which I will call "Northern Nostratic".

The special value of pronominal forms as markers of "genetic" affinity results from the fact that they are a peculiar part of speech in most languages. For one thing, they tend to be lost and replaced at a slower rate than most other linguistic features, often lingering after much of the respective vocabularies, and even morphological systems, have been lost and independently replaced. This makes them useful as markers in cases where the time depth is too great to make possible the reconstruction of parent languages on the basis of lexical comparisons, which is the customarily demanded requirement for relatability.

It is true that pronominal markers are sometimes reshaped, but in the process the new markers are often reconstructed out of old pronominal material. This accounts for the effective longevity of these markers.

Another valuable feature of pronominal forms is their relative "purity" as markers, for they are seldom borrowed from one language by another, often being more stable than even bound grammatical elements. Exceptions are known, of course. For example, Comrie mentions a case where an Aleut language in Siberia has adopted Russian verbal terminations, and I cited one or two examples in my pamphlet. But such instances are peculiar in that they seldom occur except in cases where the recipient language is moribund because of overwhelming dominance by the donor language under conditions of intense bilingualism. Most of the extant languages in Nostratic do not fall in this category since moribund languages are, by definition, languages which do not survive -- except in those unusual cases where the period of morbidity is interrupted by changes in the social or geographical circumstances.

This relative stability of pronominal forms seems to be due to the fact that pronouns have a peculiar syntactic force in most languages. For one thing, they tend to be definite (as opposed to indefinite), and this may be related to the fact that their declensional forms are often different from those of nouns. For example, in early IE and Uralic some of the pronouns had a nominative singular ending in -t (-d in IE), which was not shared by the nouns. Also, in both families the plural in -i appeared in the nominative case of pronouns, but in the nouns it was restricted to the oblique cases and preceded the case endings.

Of course, pronominal elements, whether bound or unbound, are like other linguistic features in that they undergo phonetic changes. Luckily, in the majority of cases the changes are such that the new forms are pronounced in the same locus of articulation as the old, assuming there has not been an outright loss or replacement. Thus the articulation of consonants may fluctuate back and forth between full stops and spirants, and voicing may *fluctuate between fully voiced and unvoiced, yet the locus of articulation remains constant in the majority of cases. This is a fortunate circumstance since it increases the comparability of pronominal systems in languages which are only distantly related, if at all, since it allows us to categorically combine consonants which share a common locus of articulation, without regard to voicing or the precise nature of the closures. This principle was applied in setting up the phonetic categories in the statistical analysis in my pamphlet.*

The principle has well known exceptions, of course. For example, dentals in palatal surroundings may become sibilants; velars in like environments may be palatalized to dentals with closures ranging from stops to affricates, spirants or sibilants; velars in labial surroundings may become labials; and nasal consonants may be denasalized. This all adds to the "noise", or uncertainty, involved in comparing the pronominal systems in otherwise disparate languages. The function of an a posteriori statistical analysis is to enable us to cut through the noise, to a considerable degree, to determine whether the apparent agreements are sufficiently numerous to justify our taking them seriously as indicative of a genuine historical connection. In the case of the three Eurasiatic families the answer is overwhelmingly in the affirmative.

Where does this leave us? Clearly, it means that the same process of linguistic spreading and diversification, abundantly observed in historical times and easily inferred for late prehistoric times, must surely have been going on in earlier prehistoric times as well. This is not in the least surprising. After all, when we look around us in the world today we see that all populations of humanity, no matter how we may wish to categorize them as "races", consist of only a single genetic species -- Homo Sapiens. We know this from the simple fact that all human populations are able to interbreed, without restriction, yielding fertile and vigorous offspring -- the most fundamental and obvious criterion of species identification. We seem to be the only conspicuously visible genus of animals that has not undergone unarrested speciation during the last few million years, for the fossil record shows no sign of it since the earliest appearance of Homo Erectus, and apparently even farther back to Homo Habilis. Does this mean that humanity has somehow become immune to the selective pressures of evolution and has thereby escaped the divisive effect of speciation? Of course not. It merely means that the trend toward speciation has consistently been overridden by a constant and ubiquitous process of mixing and remixing, involving out-crowding and including occasional exterminations. But in order to mix it is necessary to meet, and this means that man has been egregiously and consistently *peripatetic throughout his existence as a separate genus, filling and refilling the Earth's terrestrial surface throughout its accessible and habitable expanse. On the whole, the expansions have mainly been radiations from the interior of continental masses outward, illustrating the general principle that "the weaker members are pushed to the wall". Occasionally, as opportunity offered, this outward pressure has caused a repeated spilling-over of populations onto other continents. The concomitant languages must likewise have been in a constantly repeated process of spreading ever since languages were first devised.*

Of course this picture of outward spreading must be modified by taking into account the effect of internal geographical features presenting varying degrees of impediment to migration, such as mountain ranges and deserts, which make possible the temporary and partial isolation of relict tribes and their languages.

But this outward spreading of populations is not the only significant factor in maintaining the unity of humanity as a single species; the same outward trends are observed in every other mobile species as well, and for the same reasons. What makes our species unique is the intensification of our intellectual faculties which makes possible the invention and transmission of technologies and of cultural traditions which sustain the technologies, and which present an effective balance between stability and adaptability. Stone tools, for example, have been made since Homo Habilis, and fire has been controlled since Homo Erectus. Thus man has been a machinist for about two million years, and a chemist for almost a million.

It is this intellectual power, together with our tree-bred dexterity of hand in conjunction with eye and brain, that enabled our ancestors to dethrone the primacy of the senses -- especially the sense of smell, already attenuated in our simian cousins. This combination has given us a detachment, a coolness in confrontations, which has led to a second great principle: "If you can't lick 'em, join 'em." Thus conquests have seldom been complete and permanent in their effects, for the conquered are often enlisted with the conquerors, and it has been demonstrated many times that the meek often do inherit the earth. Individuals in tribes broken by war, starvation or pestilence have often perished, but also have often been saved, whether as slaves or as honored refugees. They may initially be despised, but after several generations their descendants are generally accepted as equals.

There is yet a third factor involved in the maintenance of mankind as a single species. It is a generally recognized prin-

ciple of evolution that no two species of plant or animal can permanently share a single ecological niche except in some cases where there is a close symbiotic relationship, and in the non-human world of living creatures the niches are often very narrowly delimited, providing opportunity for the simultaneous existence of closely related but genetically isolated species in the same territory. But the intellectual power of our own species has given us a versatility which is enabling us to occupy many niches at once. Thus no other large animal can compete with us since we can usurp any niche which is accessible to such an animal, whether human or non-human primate, or other; there is no room on this planet for a genetically isolated separate species of humans, and there is no prospect that this situation will change until human beings manage to settle on planets of other star systems.

These three factors, operating together, are the causes why mankind is still a single species and may be expected to remain so as long as we occupy a single planet.

In view of all this, it is clear that the existence of a far-flung catenation of "genetically" related families of languages in a comparatively unrestricted region like Eurasia should be the rule and not the exception; it would be surprising indeed if this were not the case, particularly since the vast East-West expanse of Eurasia can accommodate enormous migratory distances with little change in geographical latitude. At the same time, it is necessary to recognize the fact that the above described outward spreading of tribes and their concomitant languages has not been an uninterrupted process. In fact, judging from historical developments, we see that the pattern has often been dominated by relatively parochial expansions of more limited range, generally due to local advances in technology and/or social organization. Yet even parochial expansions had distant effects, for when nearby tribes were displaced they, in turn, often displaced their neighbors on the other side; this process must often have been operative in regions that had not yet been urbanized, which was the situation in most of Eurasia until the Christian Era (here I am excepting the SinoTibetan realm, which linguistically is not a part of Nostratic). The result has been the wide distribution of Caucasoid populations which we observe throughout much of Eurasia (shading off to Mongoloid in the East) and also in Southern and Southwestern Asia and even North Africa. In fact, except for the Mongoloid element in the East, the Nostratic-speaking populations consist largely of people of the Caucasian "race", which actually is a complicated mixture of many subraces in varying proportions. This implies that an *enormous expansion of Caucasoid populations occurred during the Upper Paleolithic period and has continued to modern times.*

Thus, a priori, the existence of a prehistoric "genetic" relationship among the linguistic families in Eurasia is more likely than not. Of course, all languages are constantly being changed by the people who use them, so it is inevitable that the overt markers of relationship should become progressively less apparent in widely separated members of the continuum. Today, only the most stable markers, such as pronominal forms, are still available in most cases.

Yet once we recognize the significance of pronominal forms as markers, we are emboldened to search for other markers which also might tentatively be considered, particularly grammatical elements. For example, Eskimo languages have a dual and plural in -k and -t, similar to what Collinder has identified for Uralic. In closely related families like Uralic and IE the grammatical similarities are particularly numerous.

These considerations further embolden us to take seriously the fact that most of the languages in Nostratic are, or have been, inflected on the basis of a common syntactic principle -- namely, the positioning of modifying elements before modified elements (the main exception is AfroAsiatic). As a result, the grammatical elements are generally suffixes rather than prefixes, and the unmarked order of words in the simple sentence has been subject-object-verb (SOV), originally even in the recently altered historically-known Post-IE languages, as Prof. Lehmann has brilliantly demonstrated in 25.1. The prehistoric syntactic pattern has been most clearly preserved in Altaic languages. On the other hand, the suffixal nature of the derivational process is seen most clearly in Collinder's description of Uralic (1.1 and 1.2, pp 255-281) and in Burrow's description of IE (2.1, pp 117-372); in both cases the suffixes consist of simple phonemic elements, usually consonants, sometimes grouped in catenations and usually separated by vowels as convenient.

Ironically, the conventionally required condition for relatability -- namely, that the languages must share a respectable body of common vocabulary sufficient to allow the formation of a consistent set of phonetic correspondences -- is simply not applicable when discussing "genetic" connections among the distantly related families of Nostratic. The reason is obvious when we consider the great time depths involved, dating from the Upper Paleolithic. The criterion can be applied in intra-family investigations since the time depths are small enough so that a substantial common vocabulary can be postulated in these cases, but for inter-family investigations the time depths are too *great to allow the preservation of enough common vocabulary. Besides, the accumulated phonetic changes are a formidable obstacle to specifying the histories of the changes with any realistic confidence, or even to identify the few cognates that still remain. Under the circumstances, it is inappropriate to insist upon an extensive comparability of vocabularies as a necessary condition for relatability when comparing the major families in Nostratic.*

To appreciate the seriousness of this difficulty, we can consider the implications of lexicostatistics. If we assume that the probability of loss of any given non-pronominal word in a particular language during any given millennium is p (whether by a significant change in meaning or by being dropped from the language altogether), then q = (1-p) is the probability that the word will be retained in that meaning throughout that millennium. In order for the word to be retained through J millennia it is necessary for it to be retained through each millennium independently, and the probability of the combined event is therefore q raised to the J-th power, which I will call q@J.

If we now consider a "genetically" related language having similar statistics, and assume that the two languages were identical at a particular time in the past, thus sharing an identical vocabulary, then every non-pronominal word in one language can be paired with its counterpart having the same form and meaning in the other language. Since the two languages have the same statistics, the probability that the same word in the second language will be retained through J millennia will again be q@J, just as was true of the first language.

In order for a given pair of originally identical words to be retained in both languages through J millennia it is necessary for each member of the pair to be retained independently. The probability of the combined event is therefore the square of q@J, or q@2J -- q raised to the power 2J -- which I will designate simply as capital Q. Conversely, the probability that the pair will be broken is (1 - Q), which I will call capital P. The pair will be broken if either or both members of the pair are lost at some time during the J millennia.

The situation is dichotomous -- i.e., a given pair is either retained or broken. Hence a binomial distribution applies, and we can state that the probability that exactly K pairs will be retained out of an initial list of N pairs is Q raised to the K-th power, times P raised to the (N - K)-th power, times C(K,N), where C(K,N) is the number of combinations of N objects taken K at a time. The mean of the distribution is NQ and the standard deviation is the square root of NQP; these values generally are not integers. It happens that the mean is usually very close to the median, the value of K for which the *cumulative distribution is nearest to 50%, and it also is close to the most probable value.*

Using the cumulative distribution, we can also determine the 95% and 99% levels, which are the nearest values of K for which the number of retained pairs will not be exceeded in 95% or 99% of cases, respectively.

I made some trial calculations using a p of 0.15, yielding q = 0.85. The following results were obtained, through 20 millennia, assuming an initial list of 100 pairs of words (i.e., N = 100), the usual length of list involved in lexicostatistical studies:

MILLENNIUM	*50%*	*95%*	*99%*
1	72	79	82
2	52	60	63
3	37	45	49
4	27	34	37
5	20	26	29
6	14	20	22
7	10	15	17
8	7	12	14
9	5	9	11
10	3	7	9
11	2	5	7
12	1	4	6
13	1	3	4
14	0	3	4
15	0	2	3
16	0	2	3
17	0	1	2
18	0	1	2
19	0	1	2
20	0	1	1

In order to get a more detailed picture I then repeated the calculations with N = 1000, getting:

MILLENNIUM	50%	95%	99%
1	722	745	755
2	522	547	558
3	377	402	412
4	272	295	305
5	196	217	226
6	142	160	168
7	102	118	125
8	74	88	94
9	53	65	70
10	38	49	53
11	27	36	40
12	20	27	31
13	14	21	24
14	10	16	18
15	7	12	14
16	5	9	11
17	3	7	9
18	2	6	7
19	2	4	6
20	1	3	5

In interpreting this, we can conclude that if we start out with 1000 word-pairs, the most probable number of unbroken pairs after the 12-th millennium (say) will be close to 20. Also, there will be a 95% probability that the number of unbroken pairs will not exceed 27, and a 99% probability that it will not exceed 31. By (say) "99% probability" we mean that if we were working with not merely a single case of paired languages being compared, but many such cases of paired languages having the same statistics, then in about 99% of the cases the number of unbroken word-pairs would not exceed 31.

We see that the effect of time is rather harsh for depths of 10 millennia or more, which unfortunately carries us no farther back than the end of the Ice Age. Of course, the validity of these results depends on the validity of the original assumptions, which admittedly are simplistic. For one thing, the millennial probability of loss, p, is probably not the same for all meanings. Also, even if this were not the case, the precise value to assign to p is debatable. This is regrettable since the value of p has a strong effect on the results, for if we focus our attention on, say, the 12-th millennium and try such values for p as 0.1, 0.15 and 0.2 we get the following with the 1000-word list:

p	50%	95%	99%
0.1	79	94	100
0.15	20	27	31
0.2	4	8	10

I believe the 0.15 figure for p is fairly realistic for non-pronominal forms (which is why I used it). In any case, the effect of time after several millennia is severe. My interest in the 12-th millennium is due to the fact that I believe that it represents the approximate time depth for IndoUrlalic. If this assumption is realistic, then it is not surprising that "there are very few common words" -- in fact, the paucity of "common words" is entirely in order.

In addition to the uncertainty due to our inadequate knowledge of the best value to use for p, we must acknowledge the fact that the above results do not include the effect of faulty recognition of cognates on the part of investigators, who inevitably would include some words which actually are not cognates, and would fail to include others which actually are.

***[(NOTE: The above calculations were done with a home computer directly calculating the binomial expressions. Very similar results may be obtained more simply by assuming a Gaussian distribution and taking the mean, NQ, as the median for the 50% level; increasing (adding to) the mean by 1.64485 times the standard deviation (the square root of NQP) for the 95% level; and increasing the mean by 2.32635 times the standard deviation for the 99% level. The results seldom differ more than one unit from those obtained when calculating directly.)]*

After pioneering work by Morris Swadish, Isidore Dyen chaired a conference on lexicostatistics at Yale University in April 1971, reported in 1973 (see 26.1). He and his colleagues concentrated mainly on testing and refining the method, refuting an earlier criticism by Chretien and reaffirming the fundamental validity of the method. A particularly interesting result was reported by Kruskal, Dyen and Black on page 41, where they told about obtaining a time depth for IndoEuropean (probably actually IE-Proper, excluding Anatolian) of 7200 years BP (Before Present, 5200 years BC). They were embarrassed by this result since it conflicted with the usual estimate of 4500 to 5000 years BP suggested by IndoEuropeanists, and they speculated as to how the discrepancy might be resolved without discrediting their method.

They need not have been embarrassed, for the IndoEuropeanists are wrong in estimating such a shallow time depth for Common IE since their result is based ultimately on the unjustified assumption that the undivided IndoEuropeans had been in possession of domesticated horses in either Southern Russia or Central Asia. Once this assumption is abandoned, we are at liberty to consider a less romantic suggestion -- namely, that the undivided IndoEuropeans (except the Pre-Anatolians) may be identified with the Linear Pottery people, on the upper bend of the Danube in Hungary, at the time they began their fan-like expansion into Northwestern, Northern and Eastern Europe. In 27.1, p 35, Jacqueline Murray writes "The Linear Pottery Culture has the widest distribution of the three Initial Colonisation cultures, and is found in Hungary, Rumania, western Russia, Poland, Czechoslovakia, Austria, Germany, Holland and eastern France. The earliest phase may be dated at around 4600-4400 BC and the latest to 3700 BC." Murray's work was done too early to enable her to take advantage of the tree ring correction of radio carbon dating, but when we consult Stuart Fleming's Fig. 3.5 (see 28.1, pp 68-69) we see that a correction of some 600 or 700 years is appropriate. This would carry Murray's dates back to the range 5300-5100 BC to 4300 BC. The earlier part of this range is in excellent agreement with the 5200 BC date calculated by Kruskal et al. by their method of glottochronology; far from being embarrassed, they should be congratulated.

8. CHARACTERISTICS of NOSTRATIC

This chapter is in two main parts. In the first part I will make some detailed references, or notes, on a work by Collinder already cited in this book, namely 1.5 (1975). The second part will include some observations concerning Eskimo-Aleut (particularly Eskimo), which definitely should be included in Nostratic, and indeed in Northern Nostratic as defined in the previous chapter.

A few remarks on Paleosiberian are added at the end.

Collinder's "Indo-Uralisch -- oder gar Nostratisch?" is essentially a short survey of findings by a number of scholars who have worked in this field. The first several pages give a historical account of findings by himself and other scholars in regard to IndoUralic; in effect, he reaffirmed and enlarged upon his findings in 1.1 and 1.4. This will not be repeated here.

The remaining few pages focus mainly upon recent work by two young Russian scholars, A. B. Dolgopol'ski and V. M. Illich-Svitych, pertaining to the major Nostratic families but not including EskimoAleut. I will include some summary notes, and also Collinder's bibliography in full.

In 1964 Illich-Svitych published the results of one investigation, and Dolgopol'ski actually three.

After considering 35 IndoUralic and UralAltic etymologies Illich-Svitych proposed the following hypothesis: That the three groups of IndoEuropean palato-velar stops developed partly by the labialization of original velars before rounded vowels, and partly by palatization of original velars before unrounded front vowels. He concluded that the original quality of the vowels was preserved in Uralic and Altaic.

Dolgopol'ski made an investigation in which he sought to prove, by probability calculations and lexical data, a historical connection among IndoEuropean, Uralic, Altaic, Chukchee (Koryak, Kamchadal), Kartvelian (Georgian, etc.) and AfroAsiatic ("Semitisch-Hamitisch").

On the basis of a manuscript by Illich-Svitych, who died in 1966, there appeared in 1971, in Russian, an "Attempt at a Comparison of the Nostratic Languages", listing the above linguistic families except Chukchee, but including Dravidian. Twelve Russian scholars, led by Dolgopol'ski, cooperated in the publication of this book. Thus the book consisted of two parts, one pioneered by Illich-Svitych, the other a continuation due to other scholars.

Collinder's attitude toward the book was rather skeptical, particularly in regard to the second part since it seemed to be based on an inadequate investigation of some of the constituent linguistic families, especially the "Hamitic" members of

AfroAsiatic. But he regarded Illich-Svitych's work as exceptionally valuable, and he excerpted the following items from it (Unless otherwise indicated, the asterisked forms are those assumed for IndoEuropean. The families in which a cognate is claimed to exist are marked by X):

The following abbreviations apply:

IE -- IndoEuropean U -- Uralic A -- Altaic
D -- Dravidian K -- Kartvelian AA -- AfroAsiatic

	IE	*U*	*A*	*D*	*K*	*AA*
Def.-Accusative -m	X	X		X		
Locative -n, -na	X	X			X	X
Loc.-Separative -d, -da	X	X	X	X	X	X
Lative -k, -kV		X	X	X		X
Oblique case or Genitive -n	X	X	X	X		X
Plural -t		X	X		X	X
Oblique Plural -y	X	X				X
*to bend (*gheub-)*	X		X			X
*to bind (*ker-)*	X	X	X	X	X	X
*to remain (*men-)*	X		X	X		X
*to burn (*pol-)*	X	X		X	X	
*Demonstrative (*ne-)*	X	X			X	X
*Dem.-Animate (*so)*	X	X				X
*Dem.-Inanimate (*to-)*	X	X	X		X	X
*thou (*te)*	X	X	X	X		X
*ice (*ker-)*	X	X	X			X
*deer (*el-)*	X		X	X		X
*big fish or whale (*qwalo-)*	X	X	X			X
*open hand (*lep-)*	X	X	X			X
to spin ((s)pen-)*	X	X		X		
*foot, sole (*ped-)*	X		X	X		
*to give, bring (*do-)*	X	X	X	X		
*girdle (*yos-, *yon-)*	X	X		X		
*home (*qwel-)*	X	X	X	X		X
*behind, near (U *taka)*		X	X			X
*ear, to hear (*kl-)*	X	X	X	X	X	X
*I, me (*me)*	X	X	X		X	
you (plural) (-te)*	X	X	X			X
*Interrog.-Animate (*qwo-)*	X	X	X			X
*Interrog.-Inanimate (*mo-)*	X	X	X	X	X	X
*Interrog.-Relative (*yo-)*	X	X	X	X	X	X
*throat, to swallow (*gher-)*	X	X		X	X	X
*to go, come (*i-)*	X	X	X	X		
*sea (*mor-)*	X		X	X		X
*mouth (U *angV-)*		X	X	X		

	IE	*U*	*A*	*D*	*K*	*AA*
*to put (*dhegh-)*	X	X	X			X
*fat, to smear (*sme-)*	X		X	X	X	X
*sister-in-law (*g'low-)*	X	X	X	X		X
	(also Yukagir)					
*deep (*dheub-)*	X	X	X		X	X
*to kill (*gwol-)*	X	X		X	X	X
*to kill (*wel-)*	X	X	X	X		
*much, full (*polu-)*	X	X	X	X	X	X
*to grow, live, tree (*bhuu-)*	X	X	X			X
*to wash (*mesg-)*	X	X		X		X
*water (*wed-)*	X	X	X	X		
*we (inclusive) (*mes)*	X	X	X	X	X	
*we (exclusive) (*ne-)*	X			X	X	X
*to break (*mer-)*	X	X	X	X	X	X
*tongue, to lick (*dngwaa)*	X	X		X	X	

Of these 49 listed traits, 45 are cited for IndoEuropean (IE), 42 for Uralic (U), 36 for Altaic (A), 36 for AfroAsiatic (AA), 32 for Dravidian (D), and 20 for Kartvelian (K). Thus Uralic, and especially IE, seem to be comparatively conservative. They are followed by Altaic and AfroAsiatic, less closely by Dravidian, with Kartvelian bringing up the rear. Several speculative conclusions are suggested by this ranking.

The apparent conservatism of Uralic and its central geographical position suggest that Central Asia may have been the center of Nostratic expansion during the late Upper Paleolithic and that IE represents a late Westward expansion of a Pre-Uralic speaking people into the Balkans. The conservatism of IE is somewhat illusory in that some of it is based on data taken from extinct languages. The same may be said of AfroAsiatic.

Altaic, Dravidian and especially Kartvelian may represent relatively early separations from Nostratic, Altaic later than the others. Kartvelian, in particular, is a member of the Caucasian languages which have been isolated in the Caucasus until modern times; that isolation seems to date from a very early period. In fact, the Caucasus and the mountains to the Southwest, South and Southeast seem to have harbored relatively isolated Caucasoid populations from very early times. We have historical knowledge of Hattic in Anatolia and Hurrian in Eastern Anatolia in Hittite times, Sumerian in Iraq in early historical times, and Burushaski in modern times. Of these, Sumerian and Burushaski may well have arisen from early Nostratic.

Dravidian, on the other hand, may represent a slightly later detachment from the Central Asiatic center, yet one which antedated the separation of Altaic. It may have become temporar-

ily isolated in the mountains Southeast of the Caucasus. In any case, we know that it eventually expanded into the Indian subcontinent. This expansion must have been relatively rapid, requiring only a few centuries; this seems to follow from the fact that all the historically known Dravidian languages, both Northern and Southern, apparently evolved from an original highly unified state wherein the ancestral language emanated from a quite restricted geographical region. It is convenient to call such a phenomenon a "focal" expansion, wherein all the "daughter" languages evolve from what is essentially a single dialect; this necessarily implies a restricted geographical homeland.

As stated above, these remarks are speculative because of the many possibilities for error. For example, the low ranking of Kartvelian may result partly from the fact that the Caucasian languages have not been as intensively studied as the other groups have.

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ABBREVIATIONS:

SSUF: Sprakvetenskapliga saellskapets i Uppsala foerhandlingar.
SuW: Collinder 1964.
UAJb: Ural-altaische Jahrbuecher.
UUAs: Uppsala universitets arsskrift.

(END of Notes on "Indo-uralisch oder gar Nostratisch?")

Collinder's bibliography makes only two references to EskimoAleut as a possible linguistic relative of some of the usually recognized Nostratic families, namely the works by

Bergsland and Sauvageot. This seems to reflect a divided attention on the part of investigators. For example, those who have been active in comparing IndoEuropean (IE) with AfroAsiatic (AA) have generally ignored any connection of IE with Uralic (U) or Altaic (A) or EskimoAleut (EA). Those comparing IE with U have ignored AA and EA. Those comparing Dravidian (D) with U or A have ignored a connection with IE or EA or AA. And those who have compared EA with U or PaleoSiberian (PS) languages have ignored its similarity to other Nostratic families. This lack of comprehensive treatment is unfortunate since it weakens the attack on the Nostratic problem. The cited Russian scholars made an attempt to remedy this situation, yet even they neglected EA.

Very few investigators of EA have made any attempt to compare its pronominal forms with those of other Nostratic families; the main exception is Bergsland (see 31.1, p 22), who included 1st and 2nd person suffixes along with a number of other items being compared with corresponding phenomena in Uralic. Bergsland made frequent references to earlier comparative work by Uhlenbeck (33.1) and Sauvageot (34.1). Hammerich (in 32.1) made a systematic comparison of the pronominal forms among the major Eskimo languages and dialects, attempting to reconstruct their Proto-Eskimo forms, yet he did not undertake a comparison with other Nostratic families.

In accordance with the statement in the beginning of this chapter, the following remarks on EskimoAleut will be restricted mainly to Eskimo. The reason for this is partly because I have little information on Aleut and partly because I suspect that Aleut is less conservative on most points than Eskimo seems to be, and hence would give less reliable information concerning ancient features. For the same reason, I will focus mostly on Alaskan Eskimo -- the book by Hinz (see 30.1) -- in preference to Greenlandic. One of the caveats in using Greenlandic is that original nasal consonants (n and m) are denasalized to t and p when final; also, original t in Greenlandic is assibilated to s when associated with original i.

The following list of pronominal endings is from the book by Hinz:

	----Absolutive----			------Subordinate-------		
	sing	dual	plur	sing	dual	plur
	(A)	(B)	(C)	(D)	(E)	(F)
my	-ka	-gka	-nka	-ma	-gima	-ma
thy	-n	-gkin	-tin	-pit	-gpit	-pit
his	-a	-ak	-ai	-an	-gkin	-ain
his own	-ne	-gine	-ne	-me	-gime	-me
	(G)	(H)	(I)	(J)	(K)	(L)
2 our	-puk	-gpuk	-puk	-miginuk	-gimiginuk	-miginuk
2 your	-sik	-gtik	-tik	-pitik	-gpitik	-pitik
2 their	-k	-gkik	-kik	-ginuk	-gkinka	-kinka
2 their own	-sik	-gtik	-tik	-miginuk	-gimiginuk	-miginuk
	(M)	(N)	(O)	(P)	(Q)	(R)
our	-put	-gput	-put	-mta	-gimta	-mta
your	-tse	-gtse	-tse	-pitse	-gpitse	-pitse
their	-at	-gkit	-ait	-ata	-gkita	-aita
their own	-sing	-gting	-ting	-ming	-giming	-ming

These are the personal endings which are suffixed to nouns to show possession; this is the simplest exposition of the pronominal morphemes which can be made. To facilitate discussion I have grouped the endings in categories identified by letters of the alphabet. These endings are much the same as those used to show the pronominal subjects of verbs; they are further suffixed by additional endings to show the pronominal objects of transitive verbs. The independent personal pronouns are of secondary origin and normally are not used except for emphasis.

EskimoAleut syntax, and the grammar which flows from it, are dominated by two principles:

1. A modifying (or subordinate) element precedes the element which it modifies (or is subordinate to).

2. With some exceptions, a noun or unbound pronoun in subordinate position, or a verb in a relative clause, is overtly marked by the suffix -m (or -m- or denasalized variants when followed by certain additional consonants).

The first of these is no surprise since it is the same as the order which prevails (or once prevailed) in the other Nostratic families. The only exception known to me is AfroAsiatic, where the opposite order is used; this suggests the occurrence of a syntactic revolution in that family similar to that which occurred independently in the Neo-Celtic languages.

The second of these appears to be unique to EskimoAleut, for it seems to be absent in all the other families. Nevertheless, as the discussion below will show, there is reason to suspect that the subordinative marker -m was once used in the other families also.

As shown in Hinz's list, the pronominal endings are divided into two kinds, the absolutive and the subordinate; actually, these designations refer not to the endings themselves, but rather to the bases to which they are attached. The endings are mostly different in the two groups, showing the profound importance of the principle of subordination in the syntax and grammar of these languages. There are six columns in the list, headed by "sing", "dual", "plur", etc., showing the number of objects possessed. Each column is vertically divided into three parts, indicating the singular, dual or plural possessor. These forms show a number of similarities to forms of similar function in other Nostratic families, as follows:

1. In Groups A, B and C (absolutive) the 1st pers singular possessor has the ending -ka with nasal variants. This is like the velar or (reconstructed) laryngeal which appears in the same function in IE and Uralic languages, not to mention other families. It is significant that it is restricted to the 1st pers singular in all three families, not occurring in the dual or plural.

2. The 1st dual and plural (absolutive) possessors (G, H and I for the dual, and M, N and O for the plural) have the characteristic morpheme -p- with spirant variants. This is like the non-nasal labial (-w- in IE) which serves as an alternant to -m- in Uralic and IE. It is significant that in all three families the non-nasal labial is excluded from the 1st pers singular except as a secondary denasalization of m (as in Altaic and in Brythonic Neo-Celtic).

3. The 2nd singular (absolutive) possessor (in A, B and C) has the ending -n. This is like the -n used for all three numbers of the 2nd person in Ob-Ugric and in the Turkic branch of Altaic.

4. The 2nd dual and plur (absolutive) possessors (same references as in 2.) have -s-, -ts- or -t-. These are like the s and

t which occur in all three numbers in Altaic and IE, and like the t which appears in Uralic and AfroAsiatic.

5. The 3rd person (absolutive) possessors have -a or -a-. This is like the vocalic demonstratives which appear in IE and other Nostratic families.

6. The 1st person (subordinate) possessors have -ma or -m- in all three numbers. This is like the m- or -m- which occurs in all three numbers in Altaic, Uralic and IE, and perhaps even in Dravidian (according to the Russians). Ordinarily, we do not think of it as a feature of AfroAsiatic, but when we look at Chadic (Hausa) we find that it does have an -m- in the 1st pers plural of verbs.

7. The 2nd pers singular, dual and plural (subordinate) possessors have -pit- or -pitse. Here the -p- is regarded as a denasalized vestige of the subordinative marker -m, leaving -t- or -ts- as the primordial significant element. This seems to be a repetition of the forms in 4. above. Incidentally, Hammerich's comparisons yielded a velar which precedes the -p-; the significance of this is unknown to me, though it may be useful to mention that a velar alternates with the t in AfroAsiatic, though not in the three Eurasiatic families (Altaic, Uralic and IE).

I will make no comment on Hinz's reflexive forms; they appear to be unrelated to corresponding forms in other Nostratic families.

In general, the subordinate forms show substantial contamination from the subordinative marker -m. Indeed, the very fact that the 1st person possessor in -m- is restricted entirely to subordinate forms suggests that its primordial historical origin may have been due to such contamination. Nevertheless, the fact that the -m- has been restricted to the 1st person (except for the reflexive, which may be secondary) shows that it is specific to that person, making it comparable to the -m- in other Nostratic families (in verbal forms the specificity is more apparent). It is this fact which leads me to suspect that all these other families may represent a later tradition wherein only this marker was preserved as a vestige from an earlier tradition which marked the subordinative in -m, which has been preserved only in EskimoAleut.

As stated above, the independent personal pronouns seem to be of secondary origin, hence unrelated to those in other families. However, some of the interrogative and demonstrative pronouns have respectively k- and t-, which clearly are related to the corresponding pronouns in the three Eurasiatic families. Eskimo also has a demonstrative in u-, similar to vocalic demonstratives in other Nostratic families.

These pronominal resemblances are enough to make it quite clear that EskimoAleut is a member of the Nostratic group of linguistic families, with closest relationship to the three Eurasiatic families. To see this more clearly, we can consider the example presented by the majority of the Australian aboriginal languages. Most of these are manifestly related to each other, and they show a syntactic system and a typology which are quite similar to those of EA, yet their pronominal forms show very little similarity to those of EA and the other Nostratic languages. Although we cannot actually prove that the Australian languages are "genetically" unrelated to Nostratic, we have to accept the fact that for all practical purposes they must be regarded as such. If they actually are related, then we can assume that the geographical separation of early Australian speakers occurred at a time which preceded the evolution of the Nostratic common language and its characteristic pronominal forms.

Similar observations, mutatis mutandis, can be made concerning SinoTibetan, MalayPolynesian, Bantu and Amerindian languages (except EskimoAleut, of course).

The syntax of Eskimo has many fascinations. Simple sentences are divided into two main kinds, those with intransitive verbs and those with transitive verbs; the distinction is important because the two constructions are fundamentally different. Both are characterized by the lack of a copula.

The so-called "intransitive verb" is best understood as an active participle, a verbal adjective, and morphologically similar to other participles except for mode signs. Its verbal force is acquired from its predicate position at the end of the sentence. It is preceded by the subject if the latter is a noun in the 3rd person; it is in apposition to it, and both are in the absolutive.

The so-called "transitive verb" in a main clause is best understood as a gerund, a verbal noun in the absolutive though generally suffixed by a pronominal object which accordingly is relatively "superordinate". It is "possessed" by its subject noun, which therefore is put in the subordinative form and accordingly precedes it. The object is definite and is in immediate apposition to the verb and likewise is put in the absolutive; it limits the meaning of the verb and hence precedes it. It is as if we could say in English "Man's bear-slaughter.", *meaning "The man killed the bear." This is the construction which is usually cited as evidence for ergativity in Eskimo.*

An indefinite object is expressed by the so-called "half-transitive" construction, which is actually intransitive in form. The "object" is not a true direct object but is instead an indirect object which is semantically and morphologically instrumental; accordingly, it is put in the modalis. It is as if we could say in English, "Man, stone's-means throwing.", meaning "The man threw with a stone." Here "throwing" is an active participle in predicate position as in normal intransitive constructions; it is modified by "stone's-means", which therefore precedes it. "-means", the modalis ending, is an abstract noun which is "possessed" by "stone", which accordingly precedes it. Each of these entities, except "stone", is in the absolutive.

This description is simplistic in that I have not taken into account the complications due to suffixed pronominal subjects and objects.

Save for its ergative sentence structure in connection with transitive verbs, EskimoAleut syntax has many similarities to that of Altaic languages and of Uralic and IE as it must have been in their earliest periods.

*Except for innovations of its own, the picture I get of EskimoAleut is that it represents a conservative retention of Nostratic linguistic structures from an exceptionally early period. The ancestors of its speakers must have been isolated in the Siberian Arctic and thus were shielded from innovating influences due to continuing Nostratic developments in Central Asia, and this isolation was continued when they entered a similar arctic environment in the New World. The isolation is further indicated by the fact that, except for borrowings from modern languages, the vocabularies of their languages have little in common with that which can be gleaned by comparison of the other Nostratic families among themselves. However, in accordance with the principle of the quasi-exponential decrease of primordial non-pronominal vocabulary through time, it is not surprising that a few apparent cognates still remain. For example, Eskimo has a root tun-, "to give", which appears to be cognate to similar forms in most other Nostratic families, including IE; the IE root *doH- (shared with Uralic, laryngeal and all) may be the result of a back-formation. Another example is ingne-, "fire", which suggests comparison with Latin ignis; the vowel in the first syllable is uncertain in IE (ig- in Latin, ug- in Lithuanian, ag- in Sanskrit, which are mutually incompatible), and it seems entirely possible that this uncertainty could be the result of a late denasalization of an original velar nasal in the first syllable. Yet another example is alla-, "other", which reminds us of IE *aly-. Still another example is qapok,* "foam", which resembles a UralAltaic word with meanings like "excrescence, swelling". Such examples are few, however, and their paucity suggests that the time depth between modern EskimoAleut and the other modern languages is at least 15 millennia. In any case, it is probably greater than the time depth among the three Eurasiatic families.

Thus it is not surprising that the latter group shows some innovations which set it apart from EskimoAleut. For example, most of the grammatical resemblances between IE and Uralic cited by Collinder in 1.1 and 1.5, though shared in part by Altaic, are not found in EskimoAleut. An exception noted by Bergsland is the locative in -ne (-me in the singular). Other resemblances were noted by Bergsland in his comparison with Uralic, including the dual and plural in -k and -t, respectively.

The "case system" of Eskimo presents some interesting problems. According to Hinz, there are only two cases, the subordinative and the absolutive ("subjective" and "objective", as he terms it). Those suffixes which would ordinarily be regarded as case endings, he calls "appositions", apparently implying that the case suffixes are in apposition to the substantives to which they are attached. But this is clearly a misnomer since entities in apposition, though syntactically subordinate (or relatively superordinate, depending on the point of view), are normally so closely associated that they have the same grammatical rank, which manifestly is not the case here. On the contrary, the substantive is grossly subordinate to its case suffix and therefore not only precedes it in accordance with the usual syntactic rule, but is overtly marked by the subordinative marker -m- when singular. The suffix is best understood as an abstract noun (meaning "location", "means", "separation", or the like) which is "possessed" by its associated substantive, which thereby is relatively subordinate. Hinz gives the case suffixes as follows:

localis	-me	-ne	point in space or time
ablative	-mik	-nik	separation or departure
vialis	-kun	-gun	instrument
terminalis	-mun	-nun	allative or dative
modalis	-mik	-nik	instrum. or sociative
aequalis	-tun	-stun	equative, similarity

The first column gives Hinz's terms for the suffixes. The second shows the suffixes for the singular, the third the suffixes for the dual or plural. The fourth gives less formal expressions for the meanings.

In Hinz's dialect the ablative has the same ending as the modalis, and such loss of the original ablative ending is typical of Western Eskimo languages, but in Greenlandic it appears as -nit (sing -mit) where the vowel seems earlier to have been a mid-high unrounded vowel since an original i would have caused the following t to be assibilated to s. In his article in 29.1 Hammerich assumes the final consonant to have been -n, which in Greenlandic would regularly have been denasalized to -t, as observed. But I am skeptical of this assumption since the forms would then be almost identical to those of the terminalis, whose meaning is quite the opposite, differing mainly in the rounded quality of the vowel -- in unstressed syllables the u of the terminalis would differ only slightly from the other vowel. Thus I prefer to assume that the final -t of the Greenlandic forms is original and that any indication of -n in more Western languages may be due to a secondary nasalization facilitated by a previous change of vowel. If this is correct, it suggests comparison with the ablative in -t which we observe in the three Eurasiatic families.

Hinz's paradigm is remarkable in two major respects:

1. The case suffixes express mostly "local" relationships. There is no hint of a nominative, an accusative, or even a genitive.

2. Most of the suffixes are marked by a nasal connective: -n- in the dual and plural and -m- in the singular. Those few suffixes having a non-nasal connective were presumably secondary.

By considering the implications of these two features, we can explain the origin of the oblique-n declensions in the three Eurasiatic families.

In the Eurasiatic families these declensions are clearly moribund, existing only as vestiges of an ancient paradigm which everywhere is in process of replacement by newer paradigms. It has almost disappeared in Uralic and in the Turkic branch of Altaic, is better preserved in the Mongol branch of Altaic, and is best preserved in IE, especially in Anatolian with its still vigorous neuter r-n declension; yet even in Anatolian it is under competition by the newer paradigms as the lack of this declension in Luwian shows -- obviously, the prominence of this feature in IE is due solely to the fact that we happen to have some comparatively old historically preserved examples of Post-IE languages. If these examples were not available, we would be compelled to rely on such fragmentary features as the difference between English "water" and Scandinavian "vatn", or that between "ox" and "oxen", and these would hardly suffice to engage our attention. It is extremely regrettable that we do not have Pre-Christian examples of languages in the other Eurasiatic families. Thus far, the origin of the ancient oblique-n case system has seemed utterly mysterious, frustrating all efforts toward a rational explanation. It is only when we examine the system in EskimoAleut that the structural basis for the ancient system begins to be clarified.

Initially, there might be some question as to whether the nasal connective was primordially n or m in Eskimo; arguments based on assimilation of consonants can be made either way, but on balance I conclude in favor of the n since there are some examples of pronominal declensions where the n is used in the singular as well as elsewhere, while the m never occurs except in the singular -- in other words, in the singular the n has been assimilated to the m in most instances. In fact, according to Hinz, the subordinative marker -m is not attached to nouns except in the singular. The fundamental origin of the n-connective is still unexplained, of course, and seems likely to remain so.

I have come to the opinion that there is no reason to regard ergativity as either an essential or a primordial feature of Pre-EskimoAleut; it applies only to "transitive" constructions and is morphologically unlike the constructions in other "ergative" languages, even the nearby Chukchee. On the contrary, the "intransitive" construction is fundamental and presumably primordial and quite non-ergative, and the fact that it is not now generally used to express a direct object is no reason to conclude that it was not so used in prehistoric times, for there is nothing inherent in the construction which inhibits such a use. For example, if we wanted to say "The man killed the bear with a spear.", we could use a construction like "Man, spear's-means bear-killing" -- a similar construction is actually observed in Aleut (see 29.1, p 106). Here "killing" is an active participle in apposition to the subject, "man", as in normal "intransitive" constructions; it gets its verbal force from its predicate position at the end of the sentence. "bear" is in apposition to the verb and limits its meaning, and accordingly precedes it. This object-verb combination is modified by "spear's-means", which therefore precedes it. "-means", the modalis suffix, is "possessed" by "spear", which accordingly is given an explicit marker expressing subordination with respect to "-means" -- all these entities, save "spear", are absolutive. In Eskimo this marker is fundamentally the n-connective, although it is assimilated by the more general marker -m when the noun is singular. Thus the present dichotomy between "transitive" and "intransitive" in EskimoAleut need not be regarded as an essential and primordial feature of the family; the "transitive" construction could well be an innovation which *usurped the expression of direct objects which formerly had been expressed by the "intransitive".*

If this view is correct, as I believe it is, we can see an obvious similarity between the inappropriately named "intransitive" construction of Pre-EskimoAleut and the oblique-n declensions in the Eurasiatic families, and particularly the neuter r-n declension of IndoEuropean. In this declension the n-connective is used in all the oblique cases except the accusative, which is identical to the nominative as in all the other neuter declensions. In the nominative and the accusative the ending is accordingly -r, though in the other neuter declensions the ending consists merely of the nominal stem (except the o-declension). It is interesting to note that in Eskimo the absolutive generally has a special ending except when bearing an operative suffix. This ending may be a vowel or -n or -k (an unvoiced velar stop), but most often it is -q (an unvoiced uvular stop, often spelled as capital -K, pronounced far back in the throat). And the absolutive verbal form always ends in -q (or -K) when unsuffixed -- i.e., when the subject is 3rd person singular and if there is no definite direct object. In Eskimo the uvular is always an unvoiced stop when final, but in Aleut the absolutive ending is a spirant, and I am suggesting that the spirant was the primordial form of this morpheme. In Eskimo the voiced uvular spirant has an r-like quality and is actually spelled as -r- (or capital -R-), and in fact Eskimo has no other r (except a doubled variant); the absolutive ending is also spelled as -r in the Aleut language cited in the previous paragraph. The unvoicing and "stopping" of this sound when final is a peculiarity of the Eskimo languages and presumably secondary.

If we then go on to assume that the r in Pre-IndoEuropean had an uvular quality (inherited from Northern Nostratic like the EskimoAleut uvular), and that this sound was similarly retained as a spirant when final, we can suggest that the -r of the nominative and accusative endings of the r-n declension is a reflex of the primordial Northern Nostratic uvular unsuffixed absolutive ending, still preserved in EskimoAleut, and consistent with the construction suggested above.

We can also assume that the -r ending of the "impersonal" verb, preserved in Neo-Celtic languages (and also the Oscan-Umbrian branch of Italic), is similarly a reflex of the uvular absolutive ending of the primordial Northern Nostratic verb, still preserved in EskimoAleut. It is generally accepted by IndoEuropeanists that the -r which terminates most of the personal endings of the ancient mediopassive conjugation was secondarily adopted from the -r of the impersonal verb. This is consistent with the view that most of the historically observable developments of the finite verb in IE and Uralic, and even in the Turkic branch of Altaic, represent secondary processes of personalization. The "impersonal" character of the EskimoAleut verb, often ascribed to it by investigators, is genuine and is due to its persistently participial nature and the lack of a copula, preserved from Nostratic times.

It should be noted that if the sentence had a definite direct object the uvular ending of the absolutive verb in Eskimo would be replaced by another suffix. Correspondingly, we should expect the -r of the IE impersonal verb to disappear in that situation. This suggests that the impersonal verb in IndoEuropean was not used when there was a definite direct object. This is consistent with the ambiguous "voicing" of the r-mediopassive verb in the historically known IE languages, despite the fact that it was capable of having a direct object.

The treatment of the genitive in EskimoAleut seems to be only indirectly related to that in the Eurasiatic families. In EA, possession is shown by a pronominal suffix when the possessor is a personal pronoun, as also in Altaic and Uralic. When the possessor is a noun in the 3rd person it carries the subordinative marker -m if singular, and the possessed again has the appropriate pronominal suffix of the 3rd person, as also in Altaic. In Uralic and Altaic the -m has disappeared (if indeed they ever had it), being replaced by -n, presumably an extension of the n-connective of the "local" case suffixes of the oblique-n declensions. The same seems to have occurred in IE, but here the -n has been supplemented by adjectival suffixes, and the -n- drops out in the later declensions (except the oblique-n declension of common gender like Latin "homo" and English "ox").

The foregoing reconstruction of Northern Nostratic is necessarily fragmentary and tentative because of a paucity of supporting information due to the relentless erosion of time. Some of the undergirding assumptions seem to be individually unprovable, and presumably will forever remain so. They are plausible, however, and their implications fit into a consistent pattern which is verified by observation. With these reservations, I suggest that the Northern Nostratic precursor of EskimoAleut, and later of the three Eurasiatic families, had the following characteristics:

1. The sentence structure was based on a dominating syntactic principle: namely, a modifying (or subordinate) element preceded the element which it modified (or was subordinate to).

2. Subordination of substantives and gerunds may have been overtly marked by a special suffix (-m) when singular. This may

have been the source of the 1st person marker in m which is found in most Nostratic families. A major reason for doubt is that there seems to be no trace of an m-subordinative form in any of the other Nostratic families; if this is indeed the primordial condition, we are left with no explanation for the exclusion of the nasal marker from the absolutive endings -- unless this exclusion was a secondary development in EskimoAleut due to attraction of this marker to the (now presumably secondary) m-subordinative forms.

3. Subordination of substantives carrying a "local" case suffix was further indicated by an intervening n-connective. This is rather well preserved in EskimoAleut but only vestigially in the three (later) Eurasiatic families.

4. Tense normally was not expressed, but mode and aspect signs were attached to verbs as suffixes and hence were superordinate to them. They are best understood as abstract nouns with meanings such as "completion", "duration", "repetition", and the like. Pronominal subjects were suffixed to these combinations and thus were still further superordinate.

5. The unmarked structure of the simple sentence was nonergative. A nominal subject, the object, and the verb were all absolutive, and each ended in an uvular spirant when unsuffixed. This explains the -r ending of the nominative and accusative forms of the neuter r-n declension in IndoEuropean, and also the -r ending of the impersonal verb, and why the "voicing" of the (later) r-mediopassive was ambiguous. It also explains why the neuter was the oldest of the IndoEuropean genders, and why the accusative of neuter nouns was always identical to the nominative. Thus the definite-accusative in -m was an innovation shared by Uralic and IndoEuropean (and by Dravidian, according to the Russians). The definite-accusative forms in Altaic languages were likewise innovations; the forms in Mongol and Turkic, at least, seem to be mutually cognate.

6. There was a dual in -k (or -g ?) and a plural in -t. These are well preserved in EskimoAleut and Uralic; the latter is vestigially preserved in Altaic, and apparently not at all in IndoEuropean -- unless the -s in IE can be regarded as due to an assibilization of an original -t as in Aleut, but this is highly speculative. The dual in IE-Proper is an obvious innovation. The oblique plural of nouns in -i-, shared by IE and Uralic, likewise seems to be a Post-Nostratic innovation, as is also true of the nominative plural in -i of demonstrative pronouns shared by these two families. It is true that Eskimo uses -i- to indicate the plural of a 3rd personal object, but this usage *is so different, and so limited, that it seems to be independent.*

It should be noted that some interesting secondary morphemic specializations have developed in the distribution of the 1st person dual/plural verbal personal markers in -m- and the non-nasal labial which alternate with each other in the Northern Nostratic families. In Anatolian the distribution seems to be without morphemic significance in the 1st plural, -w- being used except when the stem is the vowel u, while in IE-Proper the -w- is used to distinguish the dual from the plural. On the Uralic side the -w- or bilabial spirant is used to distinguish the plural from the dual in ObUgric, and in the Samoyedic languages there are other secondary specializations between the two markers.

7. There was no grammatical gender of the sort exhibited by IndoEuropean, AfroAsiatic, Dravidian or the Tungusic branch of Altaic -- obviously independent developments in all instances. However, there was a differentiation between animate and inanimate in the choice of demonstrative and interrogative pronouns.

8. Almost the entire panoply of pronominal forms shared by the three Eurasiatic families, and especially by Uralic and IE, is found also in EskimoAleut, which can therefore be assigned to Northern Nostratic. They include those cited earlier in this chapter and in the second paragraph of the previous chapter. This proves the existence of Nostratic (or at least Northern Nostratic) as a genuine linguistic family (or superfamily) in the usual meaning of the phrase, implying cultural descent from a single earlier language; in particular, EskimoAleut is a legitimate member of the superfamily. The proof is conclusive because such a plethora of agreements is not found outside that group of linguistic families which traditionally have been regarded, at least tentatively, as members of Nostratic, and because the hypothesis of common "genetic" descent is the ONLY suggestion which makes any sense in view of all that we know concerning the spread and evolution of languages; it is the uniqueness of this suggestion that makes the proof conclusive.

What about Paleosiberian? Are these languages members of Northern Nostratic? Before discussing this, we should note that this designation is essentially a geographical one, not implying a "genetic" relationship with the languages of any other group nor even among themselves. Accordingly, in order to have a useful discussion about possible relationships it is necessary to limit it to the consideration of specific language(s) within the group.

The best known of these languages seems to be Chukchee, which is said to be closely related to Koryak and more distantly to Kamchadal (also called Itelmen). The present observations will be brief and will be limited to Chukchee.

In 1962 Morris Swadish wrote an article entitled "Linguistic relations across Bering Strait", American Anthropologist 64, pp 1262-1291, in which he tried to show a number of consonantal correspondences between EskimoAleut and Chukchee together with other Paleosiberian languages. This was critically examined by Eric Hamp in his article in 29.1 in an attempt to put Swadesh's work on a firmer basis. I have the impression that both Swadesh and Hamp have been personally convinced of the reality of a "genetic" connection between EskimoAleut and the cited Paleosiberian languages, but that it had not yet been adequately demonstrated. Both noted, in passing, several pronominal resemblances between the two groups.

As one might expect, my own interest is mainly in the pronominal forms, particularly those of Chukchee. The clearest exposition of the personal affixes in this language that I have seen were presented by Comrie in his article in 44.1, Table 1 on p 231. He presents three columns of verbal conjugational forms, headed by Past I, Past II, and Present II, together with personal pronominal affixes and several non-pronominal prefixes. He paid little attention to the pronominal affixes since his interest was in the morphological and semantic development of the prefixes in connection with problems of ergativity. In my own examination of this table I note several resemblances to pronominal affixes in Nostratic families, as follows:

1. The 1st singular of Past I has -k, similar to a velar stop in Uralic (U) and EskimoAleut (EA) (with a nasal variant), and reminiscent of a laryngeal in IndoEuropean (IE). It is significant that in all cases, including Chukchee, this marker is limited to the 1st singular, not extending to the dual or plural.

2. The 1st singular of Past II and Present II has -m as in the three Eurasiatic families and in subordinative forms in EA.

3. The 2nd singular of Past II and Present II has -t, same as in U and EA and reminiscent of dental forms in IE and in the Mongol branch of Altaic.

4. The 1st plural has -mek or -muri, of which the -m- occurs in plural (or dual) forms in the three Eurasiatic families and in subordinative forms in EA.

5. The 2nd plural has -tek or -turi, of which the -t- occurs in

U, IE and EA and in the Mongol branch of Altaic.

6. The 3rd plural has -t, presumably a plural marker as in U and EA. In particular, the Past II and the Present II have -ne-t, reminiscent of -nt- in IE, presumably an independent development.

It is hard to avoid the conclusion that Chukchee and its Paleosiberian relatives are relics of Northern Nostratic along with all the other families which we have been discussing. Of course, we could suggest that these pronominal agreements are due to contamination from one Nostratic family or another, just as Comrie has cited concerning the adoption of Russian verbal terminations by an Aleut language. But until someone can show that the Chukchee agreements are due to a specific contamination of that kind, it makes better sense to assume that Chukchee is merely an independent relic of Northern Nostratic along with all these other families; it is a far less radical suggestion. This is not in the least surprising considering the intermediate geographical position of Paleosiberian.

9. The HOMELAND of INDO-EUROPEAN

Much has been published on this topic. I will ignore most of it since we can now see that very little of it has much probative value in this discussion. In general, previous discussions have been vitiated by poorly based assumptions and by a too parochial view of IndoEuropean as a linguistic family and of its original speakers as a people.

The most egregious of these assumptions is the notion that the undivided IndoEuropeans had been in possession of domesticated horses; for 200 years this assumption has been almost universally accepted as an axiom, a certitude. Yet the assumption has no evidence to support it, and while I have no doubt that these people had been aware of some variety of horse, the further assumption that the horses were in a domesticated state is quite unwarranted, as I believe I have adequately shown in an earlier chapter. Consequently, any proposition which is based solely on this assumption must be rejected. This is indeed the case with the South Russian theory of Prof. Gimbutas and her followers, for while I am quite willing to concede that horses were first domesticated by people in this region, the further claim that these people are to be identified with the undivided IndoEuropeans has no basis save for the notion that the latter had been in possession of domesticated horses, which is without foundation. Thus we are obliged to seek the IndoEuropean homeland on the basis of other considerations.

Before going on, let us pause to reconsider what is meant by the "homeland" of a family of languages. When two or more languages share several structural similarities and pronominal forms which are not shared by other languages to a significant degree, and if these features are of such kinds that they cannot readily be accounted for by borrowing, then we can assume that these languages have separately evolved from a single earlier language; we can assert that they are "genetically" related. We are justified in being confident of this result because this suggestion is the ONLY one that can adequately account for the phenomenon in view of all that we have been able to observe or deduce concerning the evolution of languages. It is the uniqueness of this suggestion that makes the result conclusive, for it meets the fundamental requirement of reductio ad absurdum, whose probative value hinges on the unique existence of a single plausible suggestion.

If the separation is sufficiently recent (say 8 millennia or less), we generally observe a fund of retained vocabulary which is adequate to allow us to recognize at least a few consistent patterns of phonemic similarities and differences. Many linguists make the mistake of limiting themselves to this definition, thus shutting themselves off from the possibility of gaining a deeper understanding of the prehistoric development of the languages they are studying. Two examples of this disability are the persistent failures of IndoEuropeanists to explain the origin and nature of gender and of the oblique-n declensions in IndoEuropean, neither of which can be explained without reference to other linguistic families; when we do make such references these phenomena become understandable. In principle, the failure of IndoEuropeanists to refer to Uralic and other Nostratic families is as unsound as if Germanicists were to try to explain the Germanic preterite-present verbs without reference to the rest of IndoEuropean.

Unless we choose to regard naturally-occurring languages as having been generated and evolved by disembodied spirits, or by invading extra-terrestrial beings, the existence of a set of "genetically" related languages necessarily implies the prior existence of a unique group of people who spoke mutually intelligible dialects of a single language which was culturally ancestral to each of the languages in the set; such a set of languages is called a linguistic family. We commonly observe that many such families exist as sub-sets in a chronological hierarchy wherein a relatively old family may contain younger families as constituent members -- and, conversely, such older families may themselves exist as related members of still older families. Obvious examples are the usually recognized branches of IE-Proper, such as Germanic or Slavic, most of which are themselves linguistic families containing constituent members -- and each of which is itself a member of a more inclusive and necessarily older family, namely IE-Proper (which excludes Anatolian). And going one step farther back in time, we recognize IE-Proper as the major sub-family of a yet more inclusive set, IndoEuropean, which also includes Anatolian.

We further recognize that the speakers of a given ancestral language must have lived in a specific territory during a specific era in history (or prehistory). Unfortunately, the information needed to enable us to determine the era and the territorial homeland is usually either entirely lacking or so vague that we are unable to arrive at reliable conclusions, and consequently any attack on the problem must depend entirely on circumstantial considerations, which often prove to be quite inadequate. The major exceptions are a few instances dating from comparatively recent historical times. For example, the spread of English in modern times has emanated mainly from East Mercian in the counties around London since the 17-th century. Spanish in the New World stems mainly from Andalusian, a variety of Castilian south of Madrid, during a similar period. The Romance languages come mainly from a minor Italic language on the lower Tiber during a few centuries following 200 BC. We can speak of these examples with some confidence because we have *direct historical knowledge of them. Unfortunately, such examples are few. Most of the great linguistic expansions have left little trace of their beginnings.*

Each of the above instances is an example of what I call a "focal" expansion, wherein a single language spoken in a restricted region becomes dominant over a much larger region in the course of a few centuries, a time sufficiently short that the language generally undergoes only minor changes during the expansion. The result is the comparatively sudden establishment of a single almost uniform language over a large area. In the course of time, indeed, it has generally happened that parochial cultures and dialects proceed to develop within this area so that within one or two millennia most of the non-contiguous dialects become mutually unintelligible. Nevertheless, even after several millennia their close relationship will remain obvious to later investigators.

***[(NOTE -- In recent times the tendency toward particularization of dialects is everywhere being counteracted by convergence due to extended and intensified communications.)]*

Thus is a linguistic family developed. I have here described a focal expansion as if it emanated entirely from a narrowly restricted territory at a specific point in time, and in retrospect the effect is as if this were true. In actuality, however, the process is generally more complicated. The initial expansion does indeed arise from a restricted region at what amounts to a specific point in time, but as the expansion continues subsidiary expansions will later emanate from secondary centers recently colonized by speakers of the new language -- we see this in the expansion of English, for example. But in a focal expansion, the secondary expansions occur with sufficient rapidity that the dominant dialects do not have time to change much during the overall expansion period, and throughout this period the communications throughout the region are maintained, by migrating sub-groups, with enough intensity that incipient dialectal differentiation is temporarily suppressed, at least in part. In retrospect, the overall effect is as if a single highly uniform language, with only slight dialectal differentiation, were suddenly imposed over an immense territory whose geographical extent is many times greater than that originally occupied.

Like the branching of a tree, the process has often been repeated. No expansion lasts forever; it eventually stops for a variety of reasons. One is that the conditions which instigated and enabled the expansion eventually change. Another reason is that the outlying members of the expanding population will eventually encounter geographical barriers or competing populations which have acquired the strength and organization needed to bring the expansion to a halt. Such was the case with the spread of Latin during the Roman Republic and Empire, for example.

A kind of stagnation then follows. In ancient times, at least, the communications within the expansion area were usually inadequate to prevent the development of new dialects, and these dialects would eventually differentiate into mutually unintelligible languages. Yet the internal interdialectal communications are by no means absent altogether, and the inevitable result is that a given dialect will evolve in such a way that it will most closely resemble the immediately adjacent dialects in every direction. Thus a continuum of related languages will develop such that each member will most closely resemble its immediate neighbor, and this pattern will persist until it is overlaid by later expansions from either internal or external sources. Until such an event occurs, and if the original expansion was a focal one, glottochronological studies will yield the result that all the languages are of about the same age. Such is the case with IE-Proper, for example, which thus can be seen to be the result of a focal expansion -- we exclude Anatolian, of course.

The stagnation is more apparent than real. As soon as a focal expansion slows down and the secondary dialectal developments begin, new expansions will tend to arise around parochial centers of population within the old expansion territory and along its periphery. The effect of the internal expansions is to absorb non-expansive intervening dialects; the result is that the surviving dialects will become more starkly differentiated from one another, becoming mutually unintelligible languages. An extreme example of a set of internal (and partly external) Post-IE-Proper expansions were the repeated Celtic expansions during the Central European Bronze and Iron Ages. A less extreme example within Post-Altaic was the extension of Mongol into Mongolia, which earlier had been Turkic. Examples within Post-Uralic are the Northward expansion of Finnic into Northern Russia and Eastern Baltic lands and Northern Scandinavia, and the Northward expansion of Ugric along the Ob river just East of the Urals.

The effect of peripheral expansions is to extend later branches of the old linguistic family into new territory where the languages either are utterly alien or are distantly related relics of earlier expansions. Examples from IE-Proper are the Northwestward expansion of Germanic along the Oder into North Germany and Denmark and Southern Sweden, the Eastward expansion of Baltic from the lower Vistula, and the Southeastward expansion of IndoIranian into Southern Russia and later into Iran and Northwest India. An example from EskimoAleut is the expansion of Eskimo from Alaska into Northern Canada.

Thus new "daughter" families may develop by either internal or external expansions, or by a combination of the two wherein an expansion develops from an initial position on the periphery of the old expansion territory and proceeds to grow both inward and outward.

A corollary of this process is that the most divergent and hence ancient dialectal divisions of a widely spread language are to be found in the general neighborhood of its point of origin where it was earliest spoken. In the case of English, for example, these dialects are clearly to be found in Britain where heterogeneous dialects derived from West Saxon, Kentish, East Mercian, West Mercian and Northumbrian, all dating from the Old English period, have been spoken until recent times. Outside of Britain, the dialects are far more uniform; this is the inevitable result of the fact that they are younger.

Using the same test, mutatis mutandis, we can show that the earliest home of Spanish can be located in Spain, of French in France, and of Russian in European Russia (in contrast to Siberia). All these examples, including that of English, can be verified by historical records.

A further corollary is that when the ancient dialects have not been geographically displaced or overlaid by new languages, the geographical position of that dialect which most closely resembles the widely spread language can be taken as the relatively restricted region where that language was earliest spoken. By this criterion, we can identify the East Mercian territory including London and adjacent counties as the place (within Britain) where most of the overseas varieties of Modern English were first spoken. And by the same criterion, we can show that colonial Spanish derived mainly from the Andalusian area South of Madrid in contrast to other parts of Spain. And we can likewise show that the Russian of Siberia has come mainly from the Great Russian area around Moscow in contrast to the White Russian area of the Northwest or the Ukrainian area of the Southwest. Unfortunately, this test is useful only for late expansions, for it usually happens that within a few millennia the original home of a given expansion will be overlaid by later expansions, destroying the information we seek. For example, if we try to apply this test to Central Europe (Czechoslovakia, Austria, Hungary) as a possible homeland for IE-Proper, we are frustrated by the fact that most of this region has been overlaid several times by new expansions during the last few millennia. The same remark applies to South Russia as a possible homeland.

A third corollary is that when a focal expansion has attained the limits of its first flush of growth and the succeeding period of quasi-stagnation begins, and new dialects develop within the expansion area so that each dialect most closely resembles its immediately adjacent neighbors, and later expansive dialects proceed to absorb relatively unexpansive dialects, thus causing the dialectal pattern to become more starkly differentiated, the surviving languages will nevertheless display special resemblances allying each language most closely with its nearest geographical neighbors, exhibiting locally specialized patterns of similarities and differences.

There are two reasons for this. One is that the original pattern of smooth spatial dialectal differentiation, wherein each dialect most closely resembles its nearest neighbors, is still retained in essence despite the absorption of some of the intervening dialects. Although the new languages become more sharply differentiated, each will still most closely resemble its nearest neighbors, and this pattern will continue until it is displaced by still later expansions.

The other reason is that, if these still later expansions are sufficiently delayed, special secondary resemblances will develop among some of the new languages due to special temporary social links which are established from time to time among the speakers of newly adjacent linguistic areas.

This is the basis for Schmidt's Wave Theory.

Thus the resemblances have several causes, as follows:

1. Retention of primordial features of syntax, morphology or vocabulary. Such retentions may be independent, involving both near and distant languages, or they may be conditioned (made more probable) by the mutual influence between adjacent linguistic areas.

2. Parochial adaptations of primordial features to new applications of syntax, morphology or semantics. Such adaptations may be independent, involving either near or distant languages, or they may be conditioned by temporary influences between adjacent linguistic areas.

3. Genuine innovations of phonetics, syntax, morphology or vocabulary. These innovations may be independent, involving either near or distant languages, or they may be shared between adjacent linguistic areas to the exclusion of other areas.

The prominence of independent causes of similar effects, whether retention or adaptation or innovation, is due to the fact that they operate on linguistic material which originally *had been identical, or nearly so. Thus they are to some degree preconditioned toward like effects. This is especially the case with retention, and somewhat less so with adaptation. Whether truly independent* ***innovations*** *of similar effects may be preconditioned is less clear, but the possibility of the transmission of a specific innovation to an adjacent linguistic area, when the social ties between the areas are temporarily close, seems indisputable -- of course, this would not come under the category of* ***independent*** *innovations. There can be no doubt, however, that independent innovations of similar effects do occur, for some can be verified historically.*

Some remarkable examples of similar apparent independent ***adaptations*** *of a primordial phenomenon are presented by the application in Post-IE of the nominative plural of demonstrative pronouns in -i to the nominative plural of nouns, particularly in non-neuter o- and (partly) aa-stems, in Latin, Greek, Celtic, Baltic and Slavic, leaving the plural in -s mainly to Germanic, IndoIranian and Anatolian. In view of the fact that the -i languages are in the majority and to some degree occupy contiguous territories, we might suppose that the application of the -i to nouns is original and that the occurrence of -s in the other languages represents independent adaptations or perhaps outright independent similar innovations. But the conventional view is that the -s in nouns and the -i in pronouns are original, and there are several considerations which suggest that this view is correct. In the first place, the occurrence of the -s in Anatolian indicates that it predates the existence of IE-Proper unless we are willing to defend the proposition that it was an independent development in Anatolian, which is hard to do. Second, the application of pronominal forms to the genitive plural of nouns in these declensions in Latin and Greek suggests that a tendency toward this sort of phenomenon is innate. Third, the general lack of -s in the nominative plural of demonstrative pronouns (except in OscanUmbrian) suggests that the -i in these pronouns is primordial. The unanimity of these implications suggests that the conventional view is correct, and a strong confirmation is obtained when we recall that a similar restriction of the nominative plural in -i to pronouns is a feature of Uralic as well. When we go beyond Uralic, however, this feature is no longer found; thus it appears that it was an innovation shared by IE and Uralic to the exclusion of other families.*

The multiplicity of the above causes, and the fact that a given effect may often be plausibly accounted for by different causes, often make it difficult to ascertain the dialectal history of a family of languages, particularly when we try to use dialectology to identify its major subdivisions.

9. The Homeland of IndoEuropean

In some families the subdivisions are fairly easy to identify. In Uralic, for example, there is a division between Samoyedic and FinnoUgric, and within FinnoUgric there is a younger division between Finnic and Ugric. But attempts to identify still younger divisions, apart from the individual languages, are more difficult.

In IndoEuropean there likewise seem to be two subdivisions, that between Anatolian and IE-Proper, and a younger one between Centum and Satem within IE-Proper. Admittedly, the Centum-Satem division is not clear-cut since various Satem languages share special traits with individual Centum languages which are believed to have been adjacent to them in earlier times. Of course, this is due to the fact that the beginning of Satemism occurred within the Post-IE-Proper expansion area after the first flush of IE-Proper expansion had culminated.

There have been persistent efforts by IndoEuropeanists to minimize or ignore the dichotomy between IE-Proper and Anatolian, but in retrospect it is clear that these efforts have failed; the original insight by Forrer and Sturtevant, to the effect that Anatolian was sui generis, representing a separate and mostly older tradition, was and remains correct. Sturtevant called these entities "Indo-European" and "Anatolian", subsumed under "Indo-Hittite". The relentless application of peer pressure has caused the term "Indo-Hittite" to be abandoned and replaced by "Indo-European", leaving a hiatus for the designation of non-Anatolian IndoEuropean; the implication has been that there is no fundamental difference between Anatolian and the rest of IndoEuropean.

However some scholars, such as J. A. Kerns and B. Schwartz in their classic "A Sketch of the Indo-European Finite Verb" (see 39.1), have maintained the essential separateness of Anatolian from all the other branches, calling them "Anatolian" and "IE-Proper", subsumed under "Indo-European". I have adopted their view and their nomenclature. For me, the major reason for maintaining Sturtevant's dichotomy is that all the non-Anatolian branches share (or once shared) several very significant innovations (or adaptations). One of them is the general adoption of the feminine in -aa as an adaptation of a neuter collective in -eH or -oH (where the doubled vowel indicates extended length and the H is a vowel-lengthening laryngeal), leaving only vestiges of an earlier feminine suffix in -ser or -sor which had considerable use in Anatolian.

Also, IE-Proper made at least two innovations in developing dual forms in both nouns and verbs, while Anatolian shows no trace of a dual in either category. Presumably, Pre-IndoEuropean once had a dual in -k (or -g ?) as preserved in Uralic and EskimoAleut, but there is no trace of this in either Anatolian or IE-Proper, so it is clear that the developments of dual forms *in IE-Proper were innovations from the viewpoint of Nostratic and apparently of IndoEuropean as well. In fact, so far as the nouns were concerned, it is apparent that more than one innovation occurred in IE-Proper; it is tempting to believe that these were occurring immediately before and during the IE-Proper expansion, for additional innovations continued to be made in separate branches. In regard to the verbs, we note that in Anatolian the 1st plural marker always had -w- except when the stem was the vowel -u-, whereupon -m- was used -- there was, of course, no dual. This is in marked contrast to IE-Proper, where the -m- was generalized in the 1st plural while -w- was generalized for the 1st dual as an innovative adaptation.*

A third peculiarity of IE-Proper was its general adaptation of -hh- verbs as perfects.

A fourth was the specialization of -nt- for use in active participles, while in Anatolian the voice was either passive or unspecified; the Anatolian use is apparently older since it agrees with the usage in Uralic. The only exception known to me is the Greek aorist passive participle in -thent-, but this seems to be an innovation in Greek; hence it does not apply to IE-Proper.

A fifth was the use of the stem -bh- in certain oblique cases in the declension of substantives and adjectives.

Other differences between IE-Proper and Anatolian were cited by Kerns and Schwartz (pages 5 to 8).

Of course this list is far from exhaustive.

Thus the non-Anatolian branches of IndoEuropean show a fundamental unity among themselves, setting them as a group apart from Anatolian. It is appropriate to give them a collective name which reflects that unity. In emulation of Kerns and Schwartz I use "IE-Proper".

This shows that IE-Proper was originally a single dialect among a heterogeneous collection of dialects, another of which eventually was ancestral to Anatolian; so far as we can tell, these two were the only dialects ever to emerge into history.

How, then, can we account for the eventual evolution of so many heterogeneous branches of IE-Proper from what was originally a uniform dialect? The answer, of course, is merely that these branches developed from secondary expansions emerging from various points within, and along the periphery, of a large area over which the IE-Proper dialect earlier had been swiftly imposed. In short, the initial spread of IE-Proper was a classic example of a focal expansion. Needless to say, the precursor of Anatolian was never a part of that expansion.

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Further justification for regarding IE-Proper as the result of a focal expansion can be seen in the inconclusive results of the dialectal studies that have been applied to this family during many years of investigation. The abiding classic work on this topic is still that by Meillet (see 8.2), despite various attempts that have been made to refute or amend him on one point or another. A recent attempt is one by Calvert Watkins in his article "Italo-Celtic Revisited" in 37.1, p 29, but he was effectively answered on most points by Warren Cowgill in 38.1, p 113. I conclude that Meillet was correct in assuming an actual especially close association between Celtic and Italic at an early period, amounting to what was briefly a single dialect in partial and temporary isolation from other dialects. In any case, everyone agrees that the association, if it actually existed, was broken soon after the culmination of the IE-Proper expansion; thereafter each member formed new and separate links with other dialects.

The same remark applies to early Germanic in association with early Baltic and Slavic, particularly in regard to the wholesale replacement of -bh- by -m- in certain case stems. This phenomenon is especially significant since it is unique within IE-Proper and because it violates the rules of phonetic correspondence. The only plausible explanation is that it developed at a time when the precursors of the three branches were a single dialect partially and temporarily separated from other dialects. Like the ItaloCeltic association, this group was broken soon after the culmination of the IE-Proper expansion. We know this from the simple fact that Germanic did not participate in the Satem developments which were shared by Baltic and Slavic.

The same applies to an early association between Slavic and IndoIranian cited by Burrow in 2.1, which obviously antedated the enormous expansion of the latter group in South Russia, Iran and India. We know that this association was located in non-Russian Europe (or at least no farther East than the Western fringe of European Russia) because we have no evidence of any Slavic influence on undivided FinnoUgric, while there is ample evidence of influence from IndoIranian (after its separation from Slavic). Consequently, the early homeland of the Indo-Iranian expansion must be placed in non-Russian Europe and at a much later period than the IE-Proper expansion; it was, in fact, another example of a focal expansion, one which was even more impressive than that of IE-Proper.

There was also an early association between Greek and Armenian, which obviously was broken before the development of Satem features in the latter, and long before the migration of Armenian speakers into Anatolia.

The association between Baltic and Slavic is peculiar. Originally, they were part of a single dialect along with

Germanic, but the separation of Germanic occurred early. The separation of Slavic from Baltic was necessarily later, but it was still early enough to keep Baltic from fully participating in the interchange between Slavic and IndoIranian. Even so, they have continued as close neighbors to the present day; this is reflected in a host of common developmental features. During the pre-Christian millennia the influence was from Baltic to Slavic, but since then the direction of the influence has been reversed.

Any discussion of ItaloCeltic generally assumes that the customarily named members of "Italic" (LatinFaliscan and OscanUmbrian) once comprised a genuine unity. There has been a tendency in recent years to emphasize the many profound differences between these two major subdivisions of Italic, differences arising mainly from independent adaptations of primordial features in regard both to vocabulary and to phonetic and even morphological details. Everyone agrees that most of these differences must date from a very ancient period. Nevertheless, I am convinced that C. D. Buck in his classic "A Grammar of Oscan and Umbrian" (see 40.1) was correct in regarding LatinFaliscan and OscanUmbrian as constituent members of a genuine unity, one which necessarily postdated the ItaloCeltic unity (which itself was genuine, though it did not last long). It is sufficient to recall that the organization of OscanUmbrian nouns into the same five declensions as Latin, and the organization of the verbs into the same four conjugations and even the same modes, unites the two subdivisions in sharp distinction from all the other branches of IndoEuropean. To me, such similarities are overwhelmingly significant since they pertain to the fundamental grammatical organization of these languages. I cannot accept the idea that these similarities were due to late convergence, for any late geographical and social contacts sufficient to cause such convergence would have militated very strongly against the development or maintenance of the observed numerous differences. In any case, it is apparent that the entry of the Italic languages into Italy was non-focal in nature; the constituent languages may well have been already mutually unintelligible.

On the whole, dialectologists have been disappointed in trying to identify secondary subdivisions of IE-Proper in a Stammbaum arrangement. The only marked exception is IndoIranian, which obviously represents a late secondary (and focal) expansion on a vast scale. BaltoSlavic may be another exception, but it dates from a much earlier period.

The same is true of Tocharian. Despite its extreme Eastern location in historical times, it is clear that its origin was much farther West; in his article in 38.1, p 73, G. S. Lane relates Tocharian earliest to ThrakoPhrygian (hence also Albanian ?) and Armenian (hence secondarily to Greek) and later to BaltoSlavic. Since these are mostly Satem languages these associations must have been quite early, predating the Satem developments. My own opinion is that the apparent evidence of contact with BaltoSlavic is illusory, representing independent retentions of ancient features; else the apparent lack of early contact with IndoIranian would be hard to account for. But perhaps Pre-Tocharian was situated immediately to the East of Pre-Armenian, etc., and thus never came into contact with Pre-IndoIranian. In any case, it is apparent that Pre-Tocharian had already separated from its ThrakoPhrygian-Armenian-Greek neighbors before the Pre-IndoIranians ever established their undeniable contact with this group.

In general, the picture that we get confirms the focal nature of the IE-Proper expansion, for the near-simultaneous existence of such early (though temporary) dialectal associations as Italic-Celtic, Germanic-Baltic-Slavic and Armenian-Greek requires the prior swift expansion of a single dialect over an extensive territory.

Now we come to the central question of this chapter's inquiry: Where was the language of the undivided IndoEuropeans spoken at the time immediately preceding the expansions which eventually led to the development of the historically known branches? To save space, time and personal energy I will stipulate that enough work has been done to justify the elimination of all except two possible regions, namely:

1. South Russia or Central Asia.
2. Central Europe and surrounding territory.

For brevity, I will refer to these regions as the Eastern Region and the Central Region, respectively.

The Eastern Region is the one that is currently favored by most scholars. This is due mainly to the wide acceptance of Gimbutas' assessment of the archeological situation in this region, to the effect that people in this area (her "Kurgan" people) were apparently the first to domesticate any variety of horse. The majority of IndoEuropeanists seem to regard this as adequate justification for identifying these people with the undivided IndoEuropeans. This conclusion is attractive to me -- as it naturally would be to anyone who accepts the IndoUralic hypothesis -- because it places the undivided IndoEuropeans in a region which is conveniently close to the usually assumed homeland of the undivided Uralic speakers.

Obviously, to make any sense, the suggestion that the undivided IndoEuropeans are to be identified with Gimbutas' Kurgan people, or with any other horse-using people in this region or elsewhere, requires the assumption that the IndoEuropeans had already been in possession of domesticated horses before they began the expansions which we have been discussing. But as I have shown in an earlier chapter, the assumption has no rational basis of support; its near-universal acceptance is due solely to the encrustation of academic opinion during the past two centuries. I am quite willing to allow that some of the Kurgan people may have spoken one or more languages of "Indo-European" origin, but this is very different from saying that they should be identified with the ***undivided*** *IndoEuropeans.*

Therefore let us give Gimbutas' suggestion a quiet burial, which it deserves. Is there, then, any reason at all why the Eastern Region should be regarded as the IE homeland? I have been unable to discover any. But for the purpose of argument let us assume that it actually was the homeland at the time under discussion. Let us deal with various considerations.

For example, several scholars of Dravidian have suggested that it is related to Uralic and/or Altaic because of certain resemblances which have seemed important to them. Now if Indo-European had its homeland in the Eastern Region it would have been geographically intermediate between Uralic and Dravidian, and Dravidian should accordingly show distinctly more similarity to IE than to Uralic (just as IE shows more similarity to Uralic than to Altaic since Uralic is the intermediate member of these three). But I am not aware that any scholar has ever suggested a relatively close relationship of Dravidian to IE; on the contrary, the correspondences have been almost exclusively with Uralic or Altaic, not to IE. This militates against the Eastern Region as the homeland of IndoEuropean.

The same result emerges when we consider the pattern of the influences which various IE languages have had on Uralic languages. It is well known that Uralic consists of two major subdivisions, Samoyedic and FinnoUgric, the latter being later divided into Finnic and Ugric. Many Norse loanwords have been found in Lappish (most closely related to BaltoFinnic), and many Germanic and Baltic words in other Finnic languages, but these have no bearing on the matter. More pertinent are the many IndoIranian words which were borrowed by Common FinnoUgric. Yet even these do not pertain to the period of Common Uralic and Common IE (which probably were roughly contemporaneous). Scholars have investigated the possibility of the occurrence of loanwords into Common Uralic, but these seem to be entirely lacking since none are found in Samoyedic languages -- this is a point which Collinder has repeatedly made. Such lack of influence from early IndoEuropean is strange, especially when we real-

ize that there are no significant geographical impediments to North-South communications in Russia; it militates very strongly against the Eastern Region as the IE homeland.

*The same applies when we consider the few words in Common Uralic which seem to relate to a Neolithic or Eneolithic culture -- farming, herding or the use of metals. In 1.3, pp 405 - 410, Collinder lists several hundred words which he and other scholars have found to belong to the Common Uralic vocabulary by rigorously testing words occurring in both Samoyedic and Finno-Ugric against the phonetic rules which these scholars have developed. Admittedly, these words were probably only a small fraction of the number of roots which were actually used in Common Uralic. However, there are enough of them to give us a fair-sized sample, and I have no reason to doubt that the sample is sufficiently random to give us a reasonably accurate idea of the sort of culture to which their meanings could relate. As expected, the overwhelming majority of the words seem to pertain to a Mesolithic culture; only about a dozen have meanings which might possibly have relevance to farming, herding or the use of metals. As reconstructed by Collinder, these words are (approx.): *shurte, to weed; *singe, flax-comb; *mingke, flail; *soma, linen cloth; *nile, small storehouse; *kacha, basket; *kore, basket; *pocha, domesticated reindeer; *cake, snowfield trampled up by reindeer; *soda, group of mares in heat; *pata, cooking pot; *cangcu, unmelted residue on melted butter; *waska, copper. See the referenced pages and pp 37 - 42 in 1.3 for an accurate guide to the pronunciation of these words. My representation is grossly inaccurate.*

*In Collinder's FinnoUgric and IndoEuropean lists in 1.1 we can find over a dozen words which seem to relate to a Neolithic or Eneolithic economy. Contrary to expectation, however, most of these words seem to be dissimilar to the corresponding Indo-European or IndoIranian words, suggesting that they were either formed from native FinnoUgric or Uralic roots, or else borrowed from some non-IE language. Such words include *itae, germinate; *kancko, burn-beaten clearing; *mele, cherry; *wolne, tin; *pengerae, wheel; *saente, wheat; *towke, sowing; *uche, sheep; *wede, cattle; *wuerke, udder; *raekke, porridge. As before, see the referenced pages for a guide to pronunciation; here I have used ae and ue to represent double-dotted a and u.*

It appears, then, that the Uralians and FinnoUgrians received the bulk of their Neolithic and Eneolithic acculturation from non-IndoEuropean sources -- before, during and after the separation of Pre-Samoyedic. Presumably, this acculturation arrived from the South. If this is true, it suggests that South Russia was not occupied by IndoEuropeans until a later period, for it is hard to see how they could have failed to be the intermediaries in this acculturation, which presumably would *have entailed the borrowing of a number of the associated terms. As before, this militates strongly against the Eastern Region as the IndoEuropean homeland.*

Again, if the IE homeland were in the Eastern Region, we ought to see the most anciently divided dialects of IE in that region. On the contrary, ***all*** *the IE languages that have ever been found there (Tocharian, Baltic, IndoIranian, Gothic, Slavic) are demonstrably intrusive from the West or Northwest, from non-Russian Europe. Admittedly, Tocharian was a quite early intrusion, but it was nevertheless a post-expansion phenomenon as its special resemblance to ThrakoPhrygian and Armenian (and I believe to Greek) shows. Thus, from dialectal considerations, the Eastern Region again fails the test.*

These are all the considerations I can think of which pertain directly to the Eastern Region. So far as I can see, there is ***NO*** *reason why it should be regarded as the IndoEuropean homeland.*

Let us turn now to the Central Region. Here the picture is entirely different once we evict the incubus of horse domestication. It is in this region that we find most of the truly ancient divisions of IE-Proper; the only exception is Tocharian, yet even here we can show that it originated in the Central Region and that it separated early. In fact, the antiquity of the branches of IE-Proper in the Central Region is so obvious that it is almost tiresome to discuss it. I will deal with it only briefly.

The great age of the IE-Proper unity has been grossly underestimated by IndoEuropeanists. Most of them seem to think of it as dating from about 3000 BC or even 2500 BC. But this is much too recent. It is embarrassingly close to the date that must be assumed for the separation of IndoIranian from the rest of Post-IE-Proper. It is quite true that the separation of Indic from Iranian was comparatively recent as the close similarity of Avestan to Vedic shows, yet even here we see the cumulative effect of many important phonetic differences; it is hard to see how that separation could have been later than 1500 BC. In the centuries prior to this we have to accommodate the influence of IndoIranian on undivided FinnoUgric, the leveling of the vowels e,o,a to a (long and short), and the necessarily earlier connection of IndoIranian with Slavic, which by this time had already partly separated from Baltic. We have to go still farther back to accommodate the spread of the characteristic Satem phenomena into at least Pre-Slavic and Pre-IndoIranian (not to mention Pre-Armenian, Pre-Albanian and Pre-Baltic), and the still earlier delabialization of labiovelars which necessarily preceded them in a general reduction of velarization which was of universal application throughout the Pre-Satem dialects. And *we have to go still farther back to accommodate the existence and the still earlier establishment of the Germanic-Baltic-Slavic unity. These steps were necessarily consecutive, and any one of them could easily have required several centuries for its development.*

The high antiquity of IE-Proper is most obvious in the prehistory of IndoIranian as above cited. Yet everywhere we look, in the Central Region, we can see signs of great age in the historical remains of IE-Proper. In the prehistory of Latin, for example, we have to account for the apparently ancient division of Latin (and Faliscan ?) from OscanUmbrian, and the earlier Italic unity, and the still earlier close association of Italic with Celtic. In the case of Greek we see a complete loss of the r-mediopassive and a reorganization of the verbal morphology to resemble that of IndoIranian. In almost every branch we see the often revolutionary effects of changes which have accumulated over a very long period of time.

I will not belabor the matter further except to call attention to recent work by several scholars (Bomhard, Colarusso, Dunkel and Gamkrelidze in 41.1, pp 351 - 571) who are proposing a truly revolutionary reorganization of the primordial Indo-European (and IE-Proper) consonantal system, based on typological studies. In particular, they suggest that the classically proposed voiced stops were primordially unvoiced as in Germanic, Armenian, Tocharian and Anatolian (originally glottalized); the implication is that the unvoiced character of these stops was independently preserved in these branches (after deglottalization) and that the voicing in all the other branches was the result of convergent phonetic changes radiating from a central region in the Post-IE-Proper expansion area. I have not studied the work of these scholars sufficiently to enable me to draw firm and detailed conclusions of my own, but if they are right (as I suspect they are), we have yet another example of the profoundly revolutionary effect of changes which have accumulated over a vast period of time, a period which necessarily postdated the expansion of IE-Proper.

The foregoing considerations militate strongly against any such recent date as 3000 BC for the IE-Proper expansion as usually assumed. A date of 5000 BC is far better, and this is close to the 5200 BC date found by Dyen and his colleagues in their glottochronological study cited at the end of my chapter entitled "Foundations of Nostratic". It argues very strongly in favor of the Central Region as the homeland of IE-Proper.

Another consideration is the fact that the most ancient dialectal associations within IE-Proper imply early geographical orientations which were roughly the same as those found in the Central Region in historical times. Obvious early examples are

Celtic-Italic, Germanic-Baltic-Slavic, and Greek-Armenian-Albanian-Tocharian. Later examples are Baltic-Slavic and (still later) Slavic-IndoIranian. Tocharian, Armenian and IndoIranian are exceptional in that we find them outside the Central Region in historical times, but we have reason to believe that they all originated in the Central Region. All the others, except Eastern Baltic, apparently were never outside it until the Christian Era, and in historical times we find them occupying territories which have the same relative geographical orientations as those implied by the most ancient dialectal associations. This is readily understandable if we assume that the expansion of IE-Proper occurred within the Central Region. But if we assume otherwise, we are faced with having to explain how all the non-exceptional dialects could have been translated into the Central Region from outside without disturbing their original relative geographical orientations, which is most unlikely. Again, this argues strongly in favor of the Central Region as the homeland of IE-Proper.

Another consideration is the fact that Central and East-Central Europe are remarkable for their scarcity of river names of non-IE origin, while such names occur in considerable numbers (mixed with IE names) in Western Europe and in the Southern and Northern fringes of Europe (see 42.1). This suggests that the occupation of the Central and Eastern parts of the Central Region by speakers of IndoEuropean and related languages is profoundly old; it is in marked contrast with the situation in the United States, for example, where river names of non-English origin abound despite the fact that the prevailing language is overwhelmingly English, and despite the fact that the native population has been largely replaced. It also suggests that the earliest speakers of IndoEuropean are not to be identified with the Nordic peoples of Northern Europe.

The foregoing considerations are unanimous in their implications: The homeland of IE-Proper must be removed from the Eastern Region and must instead be located in the Central Region.

What about Anatolian? The followers of Gimbutas would have us believe that its speakers entered Anatolia from the Northeast; this has an initial plausibility since this entry point is conveniently close to the Eastern Region. When we look at the matter more closely, however, the plausibility disappears, for it is precisely in these parts that we have no evidence of Anatolian speakers, while every other part (except possibly the extreme North or Northwest during the late period of the Hittite Empire) shows unmistakable evidence of occupation by speakers of *one Anatolian language or another. In fact, in the extreme West there is abundant evidence of interaction in early times between Greek and Anatolian (mostly Luwian, but not entirely) as evidenced by many old Greek place names of Anatolian origin -- see the remarks of L. R. Palmer on this topic, particularly those in his "The Greek Language"; this seems to have been due to a Westward reflux of Anatolians onto Pre-Greek territory. On the other hand, we have no evidence of Greek occupation of Anatolian territory until a much later period. The implication is that when the Greek speakers entered Greece they found the land already occupied by Anatolian speakers who thereupon adopted the Greek language. In short, there is every indication that the Anatolian speakers entered Anatolia from the West, presumably from the Southeastern Balkans. There is nothing strange about this; it is compatible with the view that the Armenians, Phrygians and Celts likewise entered Anatolia from the West.*

Thus all the evidence converges unanimously to a single conclusion: The homeland of IndoEuropean was in the Central Region.

***Where** in the Central Region? In order to answer this question we need to identify some part of the Central Region where all the following requirements are met simultaneously:*

1. There should be an archeologically identified Neolithic culture which shows unmistakable signs of swift expansion over an exceptionally large area. This is needed in order to account for the focal nature of the initial IE-Proper expansion.

2. This culture should show uniform characteristics throughout its territorial expanse, at least in its earlier period. Again, this is needed in order to accommodate the focal nature of the IE-Proper expansion; since the identity of a specific dialect is a cultural phenomenon, uniformity of dialect entails uniformity of material and artistic remains as well, at least during the earlier part of that expansion.

3. The dating of this culture should begin about 5000 BC or a little earlier. This is needed to account for the obviously high antiquity of the individual branches of IE-Proper. It also is needed to accommodate the glottochronological results of Dyen and his associates.

4. The material remains of this culture should show little or no evidence of organized warfare -- few fortifications, for example. This is needed for compatibility with the fact that the vocabulary relating to warfare is almost entirely different in the separate branches of IE-Proper (see Buck in 40.2). In *fact, it is a corollary of the focal nature of the IE-Proper expansion, which under early Neolithic conditions implies a swift expansion into an ecological vacuum where there was little effective opposition.*

5. The geographical position and orientation of this culture should be such as to account for the later existence of derivative cultures, within its original domain and along its periphery, which may be tentatively identified with historically observed branches of IE-Proper with some degree of plausibility.

6. At the Southern boundary of this cultural domain there should be archeological evidence of several older Neolithic cultures, each recognizably different from the above culture yet showing some resemblance to it, and each occupying substantially smaller geographical areas. This is needed in order to account for the existence of several dialects, separate from IE-Proper but related to it, one of which presumably was ancestral to Anatolian.

All the above requirements are admirably met by the Linear Pottery Culture, as follows:

According to Milisauskas (43.1, p 52), this culture was by far the most rapidly expansive of all the early Neolithic cultures in Europe. It appeared earliest near the Middle Danube in Hungary (near Budapest), but within a couple of centuries it had covered much of Northern Europe from Holland through Poland, and appeared latest in Rumania and even the Western Ukraine within another two centuries; the boundaries shown on Map II herein are conservative. This meets the first requirement.

The Linear Pottery Culture was likewise notable for the remarkable uniformity of its material and stylistic characteristics throughout its immense territory. This meets the second requirement.

The earliest appearance of this culture is dated at about 5300 BC (radio carbon as corrected by tree-ring data). This meets the third requirement.

The material remains of the Linear Pottery Culture show very little evidence of warlike activity. There were no fortifications until its latest period, if at all. Weapons were comparatively few and may have pertained mostly to hunting; whatever social tensions there may have been seem to have been dissipated in the rapid expansion. This meets the fourth requirement.

The geographical position and orientation of the Linear Pottery Culture were admirably suited for the later development of new derivative cultures, some of whose dialects may plausibly be identified with various historically known branches of IE-Proper. On Map III I have ventured to make some guesses as to the early home of some of these dialects; these will be discussed in the chapter entitled "Outline Prehistory". This meets the fifth requirement.

On its Southern edge the territory of the Linear Pottery Culture is immediately adjacent to the Northern edge of a group of older Neolithic cultures subsumed under "StarchevoKoeroes", dating from a little before 6000 BC; so intimate is this contact that we generally assume the former to have been derived from the latter. If this is true, it is not unreasonable to suggest that at least some of the Northern dialects of the Starchevo-Koeroes cultures may have been related to that of the Linear Pottery Culture, and that one of them may have been ancestral to Anatolian. This meets the sixth requirement.

Thus on all counts the Linear Pottery Culture meets the requirements perfectly. It has by far the best claim for identification with IE-Proper in its focal expansion. And the nearby StarchevoKoeroes cultures likewise provide a plausible source for the Anatolian branch. Thus all known branches of IndoEuropean can be accounted for within the Central Region and to the exclusion of all other locations.

10. SPECIAL PROBLEMS

This chapter begins a broad reconstruction of the prehistory of IndoEuropean and related linguistic families, involving the implications of the preceding chapters as I perceive them. Some problems which were not previously addressed will be discussed here.

Historical linguistics has similarities to human social and political history and to biological evolution. The reason is that all of these are evolutionary processes in that the developments occurring at any specific time, in any particular line, are strongly conditioned by the situations and characteristics existing at the time immediately preceding, despite the fact that the specific innovations are usually unpredictable. Thus the "present" at a specific point in time is an outgrowth of its immediate past.

To our macroscopic sensibilities the flow of time seems continuous. Consequently, when we seek to examine the conditions existing in a specific line at a particular point in time in order to mark the transition between notable eras, the precise choice of time often involves a good deal of arbitrariness. In the lines of human social or political history in modern times, a mere half-generation (10 or 15 years) is sometimes enough to show a significant amount of evolutionary change. On the other hand, in biological evolution a million years or so are often required.

The pace of linguistic evolution is intermediate between these two extremes, perhaps two or three centuries being required for a notable change in most instances. Of course we realize that such a figure can only be a very broad average since we have observed that in various linguistic lines the rate of change has sometimes been marked by sudden spurts of evolution. For example, Modern English has changed little in its phonemic and morphologic structure during the four centuries since Shakespeare, yet the major shift of long vowels was largely accomplished during the preceding 200 years after a much longer period of stability in the old conditions. Another example is the set of morphemic innovations which distinguished North Sea Germanic from the rest of West Germanic (e.g., the extension of the verbal 3rd person plural forms to all persons of the plural, and the transformation of the accusative plural of masculine a-stem nouns (IE o-stems) and its extension to the nominative -- perhaps -nz > -ss > -s); these changes seem to have occurred during the first few centuries of the Christian Era, setting a significant precedent for the continued breakdown of the old morphologic system which has characterized English in more recent times.

10. Special Problems

Since the conditioning of current evolutionary developments by the immediate past in any particular line is always incomplete, it inevitably happens that when the phenomena are viewed at two separate points in time, the two descriptions will be increasingly unlike as the difference between the two times is increased -- in other words, languages, like the elements in other evolutionary lines, change as time progresses, and the amount of change accumulates with time. In fact, in the diachronic evolution of a language the cumulative effect of gradual change is sometimes profoundly revolutionary since a radically different alternative mode of expression may come into being and may be selected for at any time thereafter. We have seen many examples of this in the histories of various languages.

Among the several analogies between historical linguistics and biological evolution is the phenomenon of speciation, a proliferation of species from a single ancestral source. In the evolution of the vertebrates, for example, we realize that all the amphibians evolved from a single species of land-invading fish, that all the reptiles evolved from a single species of early amphibians, and that all the mammals evolved from a single species of early mammal-like reptiles; it is convenient to regard a language as analogous to a biological species since in both cases there is a process of intra-communication which temporarily maintains the integrity of the entity -- genetic reinforcement in the latter case and social reinforcement of linguistic forms in the former. Speciation occurs whenever the communication becomes insufficient to maintain this integrity. The main defect of this analogy is that it does not provide a model for linguistic borrowing.

Of course, we are often led to argue about the details. For example, the three major lines of mammals -- the monotremes, the marsupials, and the placentals -- are so different in their reproductive arrangements that it might be argued that they must have evolved from separate species of mammal-like reptiles. The problem with such a statement is that we can never be sure as to the precise point at which a reptile became a mammal-like reptile, or when the latter became a mammal; such demarcations always have some arbitrariness about them. The three mammalian lines have enough significant peculiarities in common -- hairiness based on the same evolutionary process from reptilian scales, the provision of mother's milk from glands evolved from sweat glands (themselves a peculiarity of mammals), and details of internal anatomy -- that these three lines must converge to a single species as we go back in time; whether that species consisted of reptiles, mammal-like reptiles, or mammals is beside the point and somewhat arbitrary. The precise era during which that species existed is highly uncertain because of our ignorance concerning historical details.

It is the same with languages in their processes of change and speciation, i.e., their differentiation into separate dialects and "daughter" languages. The writing and preservation of histories -- particularly social histories, including the scientific description of languages -- is such a recently established activity that in only a few cases do we have enough information to enable us to make a plausible estimate as to the time or circumstances of a specific linguistic expansion or differentiation. These cases are mostly those which developed during the Christian Era, such as the expansion of Latin during the Roman Republic and Empire, that of Turkic in Central Asia, that of Arabic from Arabia, and those of English, French, Spanish, Portuguese and Russian in modern times. The main exception for Pre-Christian times is IndoEuropean (especially its major subdivision, IE-Proper), which can be pinpointed to the Northern Balkans and Central Europe in the 7th to the 5th millennia BC, as I have shown in the previous chapter.

It is different with the other Northern Nostratic families. Until the Christian Era their speakers have been so isolated from literate urban civilizations that any knowledge of their conditions in Pre-Christian times can be inferred only circumstantially from modern observations, which unfortunately are too remote chronologically to be of much help. It is as if we were faced with having to reconstruct the genealogical history of the carnivores without any reference to fossils, relying solely on the comparative anatomy of presently existing species. We could do fairly well within such recent speciations as the canids and the felidae, but relating these groups chronologically to each other and to such creatures as hyenas, weasels and seals would be far more difficult; we could do little more than assert that they all evolved from a single earlier species. That they did so becomes clear when we compare them with more distant relatives like cows, people, birds, snakes, frogs and fish -- all of these, being vertebrates of various kinds, have had a common ancestry. Yet it is clear that all the carnivores are more closely related to one another than any of them are to any of the other vertebrates; when we say "more closely related" we really mean that their initial speciation was a more recent event than the speciations which separated them from their more distant relatives.

We have the same problem with the Northern Nostratic families. It is true that in many cases the younger separations are sufficiently distinct that we can assert relative degrees of relationship without equivocation. Within IE-Proper, for example, we can state that any Germanic language is more closely related to every other Germanic language than to any language outside that group, such as Irish, Greek or any Slavic language. Likewise, we can say that every branch of IE-Proper is more *closely related to every other branch of that group than any of these is to Anatolian, and that every Anatolian language is more closely related to every other Anatolian language than to any language outside that group. We can make similar remarks, mutatis mutandis, concerning the major subdivisions of Uralic and EskimoAleut. Such cases are clear-cut, unequivocally implying relative chronologies of separation. In most cases, however, we cannot be precise concerning the absolute time and place of the separations -- the historical information needed for such determinations is lacking and presumably will forever remain so, and the circumstantial indications from comparative linguistics and archeology are often too equivocal to be of much help after the lapse of several millennia of unrecorded events.*

The problem is intensified when we go back to the undivided Northern Nostratic families themselves. It is true that such families as IndoEuropean, Uralic and EskimoAleut are sufficiently unitary that we can easily distinguish a Uralic language from any language taken from either of the other two groups, and vice-versa, but when we consider Altaic and Paleosiberian we find that the situation is more complicated; this is due partly to the fact that not enough work has yet been done on these groups.

For Altaic the problem is aggravated by the fact that the regions surrounding Central Asia comprise an enormous expanse where there is little impediment to migration. This expanse runs from Eastern Europe to Northeast Siberia, and from the Caspian Sea almost to the Arctic Ocean. During the Christian Era most of that part of this region not occupied by Uralic languages has been dominated by Altaic-speaking peoples, mostly Turkic in the West and Mongol and Tungusic in the East; the main exception to this distribution is Yakut, an eccentric extension of z-Turkic into Northeast Siberia. It is important to remember that much of this picture is a recent development, which accounts for the fact that many Turkic languages are still mutually intelligible; in Pre-Christian times Central Asia was dominated by people speaking languages derived from Post-IE-Proper -- languages related to Tocharian after 4500 BC, and also IndoIranian languages after 2000 BC. If we assume that the initial differentiation of Altaic was roughly contemporaneous with that of IE, which seems not unlikely, we must postulate an Altaic origin dating from about 5000 BC or even 6000 BC -- after all, we have already seen that IE-Proper goes back to at least 5000 BC in Hungary, and the initial differentiation of Indo-European itself in the Balkans must necessarily have preceded this.

Of course, I have no direct evidence for the antiquity of undivided Altaic as in the case of undivided IE-Proper, but *there is certainly no reason to doubt it. Perhaps a glottochronological study will someday help to clarify this. A necessary preliminary is the winnowing of borrowed forms by application of Neogrammarian procedures. A great deal of successful work has already been done, notably by Ramstedt, Poppe (see 46.1 and 46.2) and Menges (47.1). And Miller (48.1) has lately been extending these methods and results to include Japanese. In addition, Menges in 47.1 has presented many examples of special morphologic agreements between Turkic and the other branches of Altaic to the exclusion of other Nostratic families, showing quite clearly the unitary nature of Altaic as a self-contained linguistic family. In view of all this, it seems inappropriate that some scholars should still continue to doubt the existence of Altaic as a unitary linguistic family in the same sense as EskimoAleut, Uralic or IndoEuropean -- it is the passage of several millennia of unrecorded developments that has caused the Altaic branches to assume their variegated appearance, much as in the other families.*

The Altaic pronominal forms are much the same as in the other Northern Nostratic families, with special developments. There is the familiar 1st person in m- with a non-nasal variant b-. There is also the 2nd person in both s- and t-; the former has been generalized in Tungusic and Turkic, while the latter has been generalized in Mongol. According to Miller, Old Japanese preserved traces of both forms, s- in the singular and t- (with n- as a nasal variant) in the plural. A major 3rd person form in Altaic was -i-, a demonstrative form which is obviously related to similar forms in the other families. A special feature was the prominence given to -n- in the oblique cases of some pronouns (and one class of nouns, in Mongol), clearly related to the oblique-n connective familiar in other Northern Nostratic families. Altaic also has the familiar interrogative in k-; this seems not to have penetrated to Japanese, however.

In modern Japanese there seems to be scarcely any similarity between its pronouns and those familiar from other branches of Altaic and from Northern Nostratic families generally. This is due partly to phonetic and morphologic innovations and partly to honorific substitutions, presumably stimulated by a Mafia-style system of social control wherein a virtue was made of supine obedience to self-centered war lords in all their murderous ferocity.

Susumu in 49.1 also regards Japanese as Altaic, with closest relationship to Korean; this relationship is especially evident in the Japanese of the 8th century AD, as we might expect. He believes that a Korean type of Eastern Altaic was first imparted to Southwestern Japan by immigrants from South Korea who simultaneously introduced rice cultivation, a culture which is called Yayoi and which eventually superseded the much older

Jomon culture. This was surprisingly late, about 300 BC, so it is strange that Japanese should be so different from other Altaic languages, even Korean. Apparently Pre-Japanese was already well differentiated from contemporary Korean before it was introduced into the islands, or else it changed very swiftly before it was first recorded in the 8th century AD. Like English in relation to Germanic, both Korean and Japanese are Altaic in syntax and morphology and even in vocabulary; yet also like English in contrast to Germanic, they both show certain characteristics which are distinctly non-Altaic. Specifically, the names for parts of the body seem to be of MalayPolynesian origin, evidently reflecting an extensive intermarriage with the Pre-Altaic population. Thus it appears that the Altaicizing of Korea and Japan was never as thorough as was the Germanicizing of Britain by the Pre-English and their Scandinavian successors. And during the Christian Era both Japanese and Korean have been strongly influenced by Chinese -- like English in relation to French.

Summarizing for Altaic in general terms, we can suggest that its original speakers were people of mostly Mongoloid race who had adopted an Eastern dialect of Northern Nostratic -- at what date, it is impossible to say. I think it was later than 10,000 BC, but probably earlier than 5000 BC. Whether the initial expansion was focal in nature is likewise uncertain; it may have been, but in any case it was succeeded by secondary expansions as illustrated by Poppe in 46.2, pp 146-148. The earliest split seems to have led eventually to Korean (and eventually Japanese and perhaps Ainu), the other branches remaining united as the Turkic-Chuvash-Mongol-ManchuTungus unity. The latter then split into Turkic-Chuvash versus Mongol-ManchuTungus and these later into z-Turkic versus Chuvash and Mongol versus ManchuTungus, respectively. Meanwhile the Peninsular branch, represented by Korean et al., was undoubtedly doing some splitting of its own. Such successive splitting is seen in many linguistic families and is certainly no argument against the one-time existence of Altaic as a united linguistic family.

What about Yukagir? It has been customary to classify this language with Paleosiberian because of its geographical association with Chukchee and other languages of this group, but it is now generally recognized that it is actually more closely related to Uralic than to Chukchee, etc. In fact, it is my opinion that Yukagir ought to be formally included among the Uralic languages. The reason is that it shows special resemblances to Samoyedic languages in regard to certain details of nominal declension; these resemblances are not shared elsewhere, not even with FinnoUgric languages. If Yukagir were not Uralic, and specifically Samoyedic, there would be no way to account for this except to assume morphologic borrowing, but this is unlikely because of the wide geographical separation and because morphologic borrowings are unlikely in any case. I suggest, then, that Uralic ought to be enlarged so as to be divided into the two major subdivisions, FinnoUgric and SamoyedicYukagir. Of course the fact that Yukagir does not share the Samoyedic assibilation of k in association with front vowels is not an impediment to this arrangement; it merely means that the Samoyedic assibilation was a later development than the separation of Yukagir.

Thus Yukagir can be regarded as an aberrant member of the more inclusive group, SamoyedicYukagir, like Tocharian in relation to IE-Proper, like English with respect to Germanic, or like Rumanian in relation to Romance.

Summarizing for Uralic, I imagine that it resulted from a generally Northward expansion of Northern Nostratic speakers near the Southern Urals. Of course we can only guess as to the precise time when this expansion began, presumably sometime between 10,000 BC and 5000 BC. I doubt that it was as early as the Westward penetration of Pre-IndoEuropean into the Balkans. A major reason for this doubt is the fact that the existence of Lappic languages in Scandinavia and the closely related Balto-Finnic languages in Finland, Estonia and Northern Russia cannot be much older than 2000 BC. This is obvious when we recall that these groups are members of FinnoUgric, which was still undivided when the IndoIranian speakers moved from Eastern Europe into Central and Southern Russia. Of course, a Northward movement of Pre-SamoyedicYukagir speakers East of the Urals presumably preceded this.

In this connection it is interesting to speculate as to what the linguistic affinities of the Nordic peoples of Scandinavia and nearby Baltic lands may have been in Mesolithic times -- i.e., before they adopted the Neolithic way of life. I have read suggestions that it might have been FinnoUgric, but this could hardly have been the case in view of the late penetration of Finnic into this region from the Eastern part of European Russia. Further, in view of the implications of an earlier chapter, neither could it have been IndoEuropean. Consequently, our only hints concerning their languages are those words of unexplained origin in Germanic, Baltic, Lappic and BaltoFinnic languages and the non-IndoEuropean (and non-Uralic) river names in this region, which is scanty information indeed.

Although, for convenience, I have not classed Dravidian with Northern Nostratic, Menges and some others have suggested a relatively close relationship with Uralic and Altaic, hence more distantly to IndoEuropean by implication. My own reaction has been distinctly less sanguine because of a paucity of pronominal agreements with other Nostratic families. Of course this need not imply the lack of a historical connection since special developmental processes in Pre-Dravidian may have accelerated the loss or transformation of most of the familiar Nostratic pronominal forms, as in Japanese. The significance of this consideration may perhaps be overridden by a relatively large number of lexical and morphological agreements, and it is on the basis of these considerations that these scholars have formed their opinion. Also, the syntactic rules of Dravidian are similar to those of most other Nostratic families. For myself, because of my ignorance concerning Dravidian, I withhold judgment.

The precise nature of the relationship between AfroAsiatic and the other Nostratic families is even more puzzling. A recent, and I believe exceptionally significant contribution, was provided by Bomhard with his article "Indo-European and Afroasiatic" in 41.1, pp 351-474. He has adopted what he calls an "integrated diachronic-typological" approach to the problem of reconstructing the consonantal systems of early IndoEuropean and related families, earlier proposed by Gamkrelidze, Hopper and Ivanov. This approach is based on the results of typological studies of far-flung disparate languages, not necessarily "genetically" related. A salient result is the observation that unvoiced glottalized stops are of frequent occurrence except for the labial member (p'), which is rare; the implication is that these phenomena are due to universal tendencies resulting from the inherent physiology of the speech apparatus of all races of Homo Sapiens. And in view of the rarity of IE *b (as traditionally proposed), it is suggested that the usually reconstructed voiced stops in IE were actually unvoiced glottalized stops. Of course there are also other important results. My study of this matter has been superficial, but so far I have not been able to deny the force of the logic which underlies the new approach.

Bomhard provides his own new reconstructions of the sound systems of both IndoEuropean and AfroAsiatic, and on this basis he freshly proposes more than 200 separate cognate roots. Most of the meanings associated with these roots seem to be of elemental character and thus suitable for inclusion in a "basic" word list such as is used in glottochronological studies. It is interesting to note that a few of the meanings seem to imply an early stage of Neolithic culture (though not necessarily). As reconstructed by him for IndoEuropean, these include *bares- (*bhares-) "grain", *ber- (*bher-) "to twist or braid", *per- (*per-) "to scatter or sprinkle", *tauro- (*tauro-) "bull", *hhar- (*ar(e)-) "to plow", *tek(s)- (*tek(s)-) "to weave", *ger- (*gher-) "to crush or grind", *hhak'-ro- (*ag-ro-) "field", *k'en- (*gen-) "to beget", *howi- (*owi-) "sheep", *me?- (*mee-) "to reap", *lek'- (*leg-) "to gather"; the paren-

thesized forms are the corresponding traditional reconstructions. Bomhard's representations are more precise than mine.

Both in number and in kind, such a plethora of lexical agreements is in marked contrast to the 40-odd agreements that have been identified between IndoEuropean (IE) and Uralic (U) and the similarly small number between Uralic and Altaic (A). The implication seems to be that the time depth between IE and AfroAsiatic (AA) is substantially less than among the three Eurasiatic families, where the residual common vocabularies are much scantier and give scarcely any hint of a Neolithic culture, suggesting that their connections were broken before any of their speakers became acculturated to a Neolithic economy.

*One partially counterbalancing consideration is the apparent fact that Bomhard's screening of cognates was less rigorous than Collinder's in his Indouralisches Sprachgut, where cognates were not allowed except when they could be identified in several Samoyedic languages as well as FinnoUgric. For example, the IE root *wei- (*wei-) "we" is attested only in Egyptian among the AA languages, and even here the meaning is given as "I", the singular instead of the dual or plural -- such cases were not allowed by Collinder. Nevertheless, these cases are too few to make any substantial dent in the swarm of cognates presented by Bomhard. The conclusion seems inescapable that the separation between IE and AA was distinctly later than among the three Eurasiatic families.*

Why, then, are so few of the characteristic Nostratic pronominal forms observable in AfroAsiatic, and why does AA show so little of the morphologic agreements, particularly those between U and IE? -- admittedly, such agreements are far from absent, but they are distinctly fewer and less striking in similarity of application. For me, these paradoxes are the most astounding in the entire field of Nostratics.

*In several ways, IE seems to be intermediate between U and AA, just as U is intermediate between IE and Altaic (A). For example, AA shares some special features with IE which I have not seen elsewhere. As presented by Bomhard, these include *hhant- (*ant-) "front or before", *hapo- (*apo-) "turn away or back" and *?en- (*en-) "in"; these seem to have been first abstract nouns, then adverbs and preverbs in IE, and later prepositions in various languages. Bomhard also gives other examples of this kind, but these seem to be shared with other Nostratic families as well as IE. But the most cogent indicators of IE's intermediate position are the large number of lexical items, many of them shared only by IE and AA, especially those which seem to pertain to a Neolithic economy.*

10. Special Problems

In general, I draw the follow conclusions: The existence of a statistically significant number of pronominal agreements (as among the three Eurasiatic families, Chukchee and its relatives, and EskimoAleut) is a strong indication of a historical connection of some kind, and the implication of a common "genetic" origin is very likely except in a few cases of pronominal borrowing due to overwhelming dominance by a quasi-alien language under conditions which insure the borrowing of many non-pronominal words, resulting in a large fund of common vocabulary -- ironically, it follows that a paucity of common vocabulary is a strong indication that the connections were "genetic" in character. And when the pronominal agreements are supplemented by a relatively large number of shared specific morphological agreements, as in the IndoUralic case, the implication of common "genetic" descent is overwhelming.

On the other hand, a relative paucity of such agreements does not imply the converse (i.e., that the families are unrelated), especially if there is an impressive number of shared lexical items which seem to be "basic". The examples presented by Dravidian and (especially) AfroAsiatic are enough to convince me of this.

Despite the relatively recent connection between IE and AA suggested by the quantity and nature of their lexical agreements, it has to be admitted that AA shows some startling peculiarities which distinguish it profoundly from all the other Nostratic families, and which seem to date from its earliest inception as a distinct family since any lack of them in AA languages can be seen to be secondary. These include the feminine in t and a syntactic revolution whereby a modifying element follows the element it modifies, leading to a substantial prefixing of morphology. We can see that these are innovations because there are no traces of them in any of the other families except as secondary developments like the syntactic revolution in the Neo-Celtic languages.

A further reason for regarding the historically observed syntactic system of AA as secondary (from the Nostratic point of view) can be seen in the structure of words in these languages, which originally occurred by polyconsonantal suffixation as in early Uralic and IE. The prevailing tri-consonantal system in AA was an early specialization of this.

Incidentally, it should be emphasized that the old dichotomy between Semitic and Hamitic can now be seen to be false, thanks to work by Otto Roessler (see 41.1, pp 679-748, translated by Arbeitman). The above peculiarities, specifically including the prevalence of tri-consonantal roots, pertain not merely to Semitic but originally to the entire field of AA. In fact, it now appears that the original expansion of AA into Western

Asia and Northern Africa was essentially focal in nature, presumably coinciding with a swift expansion of the first bearers of a Neolithic economy into these regions -- like the fan-like Northward expansion of the Linear Pottery Neolithic into the Northern half of Continental Europe, borne by the undivided speakers of IE-Proper.

If this interpretation is correct, it would appear that the original imposition of what was essentially undivided Afro-Asiatic into the Near East and North Africa was not particularly early, for in his article on Egyptian history in the 1981 issue of Encyclopedia Britannica Eberhard Otto dates the Neolithic from a little before 4000 BC. Thus it seems to have been surprisingly late, postdating the IE-Proper expansion into Northern Europe by several centuries. Perhaps this explains why the historically known Semitic languages have shown comparatively little differentiation.

We now have to suggest a scenario which will account for the apparent facts as we perceive them. I used to think of AA as being only distantly related to IE and the other families, and I assumed that the early speakers of IE were already resident in the Balkans before they became acculturated to a Neolithic economy. The implication of this assumption is that this acculturation came to them most probably from Anatolia since it is an obvious land bridge between the Balkans and the Fertile Crescent, both geographically and ecologically, and because the archeology of the Balkans for the early Neolithic does indeed suggest a strong connection with the Aegean area. But since it now appears that the early speakers of IE must have shared their earliest Neolithic experiences with the early speakers of AA, we have to suggest a time and place where this pristine association could have existed.

Unfortunately, this seems to disqualify Anatolia as the connecting bridge, for it would require that the early speakers of AA had been resident in Western Anatolia at the time in question (i.e., before 6500 BC) so that they could serve as the intermediaries. Alternatively, we could suggest that the undivided speakers of both IE and AA could have lived near each other in Anatolia at this time. But we have no evidence of an occupation of any part of Anatolia by any AA or IE speakers until a much later period (i.e., after 2000 BC), which is late Post-IE and Post-AA, much too late for relevance in this discussion; the reference is to contact between Assyrian merchants and Hittite or Luwian princes in Eastern Anatolia. Of course, we have to admit that absence of evidence is not evidence of absence, and the mere fact that no trace of early contact in Anatolia is apparent is not enough to exclude the possibility with finality.

Still, it is enough to deter me from taking the possibility very seriously.

A second suggestion might be to assume that Pre-Semites were already established in Syria and Lebanon at the time in question and were already engaged in seaborne activities like their Ugaritic and Phoenician descendants, and that they bypassed Anatolia and imparted Neolithic techniques to Greece and Macedonia by water, thence ultimately to early IndoEuropeans in the Balkans -- we do know that various Mediterranean islands were being occupied by Neolithic settlers at this time, necessarily transported by water. This could have involved Milisauskas' Proto-Sesklo Culture, which occupied most of Greece and Southern Macedonia (see 43.1, pp 46-51, Fig 4.5). But of course I have no evidence to support this. Besides, if early AA speakers were already so vigorously expansive in the Near East in the 7-th millennium, how can we account for their apparently late appearance in North Africa? As pioneers in the spread of a relatively productive economy, they would have encountered little effective opposition.

[(Note that both of these scenarios involve the assumption that the early speakers of IE had moved Westward into the Balkans before being acculturated to a Neolithic economy, and that their AA contemporaries had become acculturated after moving into the Fertile Crescent from Central Asia, presumably near the Southern end of the Caspian Sea.)]

For our third scenario let us change our assumptions more radically. Let us assume that around 7000 BC, or a little earlier, the precursors of the speakers of both Common IE and Common AA still existed as an undivided population somewhere near the Northern fringe of the Fertile Crescent, perhaps near the Caspian Sea, having separated earlier from their closest Nostratic relatives in Central Asia, the precursors of the speakers of Common Uralic; for convenience, let us call their language Common IndoAfric. Let us assume that they now became acculturated to an elementary Neolithic economy, adopting common special terms as appropriate. Then let us assume that they separated into two main populations, the early IE speakers migrating along the North shore of the Black Sea into the Balkans, and the early AA speakers into Northern Mesopotamia, thence eventually Southwestward into Syria and nearby lands.

I assume that in the Balkans the Pre-IE speakers then diversified, developing the several StarchevoKoeroes cultures, beginning around 6500 BC, and leaving linguistic relatives of themselves North of the Black Sea. They also became influenced by further cultural impulses from Anatolia and the Aegean area.

Of course this has nothing to do with Gimbutas' Kurgan people, whose emergence was much too late for relevance in this discussion. [(I also assume that the Caspian region was less arid in those early times.)]

Meanwhile, the early AA speakers presumably remained undivided in Northern Mesopotamia for at least a thousand years, undergoing a linguistic revolution. While still undivided they developed a feminine in t, replaced much of the Nostratic pronominology and morphology, and revolutionized the syntactic rules, developing a considerable amount of a prefixed morphology and a preponderance of tri-consonantal roots; on the other hand, their retention of basic vocabulary was relatively conservative. It was not until after 5000 BC that they made their essentially focal expansion through Syria and Palestine and into North Africa.

This third scenario is the one that I now favor, for it is the only one that does not entail contradictions with respect to what seem to be the relevant facts as discernible today. [(An exception has to be made in regard to the apparent fact that the DniesterBug culture North of the Black Sea was slightly younger than the StarchevoKoeroes culture in the Balkans, which if confirmed in future investigations would invalidate the scenario. But there is enough uncertainty in radio carbon dating that I feel justified in disregarding the discrepancy pending later developments.)]

If this scenario is correct, or nearly so, it suggests that the Pre-IE language was first introduced into the Balkans about 6500 BC by people who had already been acculturated to an elementary Neolithic economy, for by 6300 we see the StarchevoKoeroes cultures already established in this region almost simultaneously with the Karanovo culture in Bulgaria and the DniesterBug culture North of the Black Sea. Milisauskas seems to regard the two Balkan groups as derivative from the Proto-Sesklo culture of Greece, which he dates from about 6700 BC. But for reasons stated above, I have to reject this and derive them from North of the Black Sea instead; it may be that the strength of this connection will become more apparent when the archeology of this region for this period is more thoroughly investigated. On the other hand, it could be that the presently apparent Neolithic connection between IE and AA is an illusion; for the moment, however, I will assume that it is not.

The penetration of Pre-IE into the Balkans may or may not have been focal in nature. But if it was, it seems unlikely that it remained united for long, for the archeological record suggests the early establishment of many geographically stable parochial settlements. In fact, we know that by 5300 the language had separated into at least two disparate dialects, one ancestral to IE-Proper, the other to Anatolian. Judging from the *heterogeneous archeological panorama, I suspect that there must also have been many other IE dialects in the Balkans at this time, unknown to history, and equally disparate or more so.*

In this connection it might be worthwhile to mention Etruscan. Attempts to analyze it have been made for many years, and specialists like Pallottino, Bonfante (see 50.1) and Pfiffig (35.1) now claim that most of its inscriptions can be successfully interpreted. This progress has been made by abandoning the etymological method and by concentration on internal evidence, correlation among inscriptions, analysis of the many loans from known nearby languages (mostly proper names), and confirmation based on a few approximate bilinguals involving Latin and Phoenician. The abandonment of the etymological method has been due to its failure to obtain consistent results, and this is due to the now evident fact that Etruscan has no obvious fundamental relationship with any other known language.

But this is actually an overstatement, for when we look at the fundamental syntactic and morphologic structure of the language as now partially revealed, we can see several similarities to Nostratic languages, and especially IE, as we might expect from geographical considerations. The syntactic rules, for example, are typical of those of most Nostratic families, wherein a modifying element precedes the element it modifies. Accordingly, the morphology is overwhelmingly one of suffixation, the verb almost always occurs at the end of the sentence except when the word order is inverted, and adjectives and demonstratives usually precede their governing nouns. In fact, the word order is much the same as in the neighboring Italic languages. The tendency of Pfiffig and his colleagues has been to suggest that such similarities were due to borrowing by Etruscan, but I see no reason why they could not be attributed to parallel inheritance from Western Nostratic.

Because of the uniformly funereal and religious nature of the inscriptions, we are left with a biased and narrowly restricted view of the language. Consequently, our knowledge of the pronominal apparatus is incomplete, and the true meaning of many words cannot be known except in a general way and with less confidence than would otherwise be justified. Pronominal forms of the 2nd person, of the 1st person plural, and of the interrogative, seem to be missing altogether. This leaves us with a 1st person singular in mi, two 3rd personal forms with "an" (animate?) and "in" (inanimate?), demonstratives in c- (k-) and t-, a demonstrative in i- or e- to which other demonstratives were suffixed, and a relative in ipa-. Of these, only the relative is alien to Nostratic (and IE), though Pfiffig attributes it to borrowing from Italic; in fact, all of these are attributed to *borrowing from one IE language or another. To me, this makes no sense.*

In the first place, we know by observation that pronominal borrowing, especially of the 1st person, has seldom occurred except in cases of overwhelming cultural dominance by speakers of the donor language, and in the case of Etruscan this condition was not fulfilled. On the contrary, through most of their history the Etruscans were among the most powerful peoples in Italy, culturally and politically; this situation did not change until the advent of the Celts in the 4th century BC. Consequently, I see no reason to assume that the bulk of their pronominal forms were not due to parallel inheritance from Western Nostratic, like those of IE. After all, if pronouns are so easily borrowed, why does everyone assume that the borrowing was all one way? People seem to forget that these forms are characteristic not only of IE but of several other families as well.

The same applies in regard to the declension of nouns in the singular. In Etruscan the accusative was identical to the endingless nominative except when definite -- as in Uralic and early IE; an exception must be made in regard to the neuter r-n declension in IE, where the -r of the nominative-accusative seems to be a vestige of an uvular absolutive ending as preserved in EskimoAleut. Also, the definite-accusative in Etruscan ended in -n, sometimes with added -i. This -n could earlier have been -m as in IE and Uralic -- the change of final -m to -n occurred independently in several languages of these families.

As in IE, the genitive in Etruscan was of adjectival origin, -s (or -s') and -l being used; in IE-Proper the -s was generalized, though in Anatolian there are also some traces of the -l. Etruscan also had vestiges of an archaic genitive in -n as in Uralic and Altaic.

The dative form in Etruscan was -i or -s'i, where the vocalic ending is reminiscent of similar dative, lative or locative vocalic forms throughout most of Nostratic, ranging from IE, through Uralic, Altaic and EskimoAleut.

The locative in Etruscan ended in -thi, sometimes shortened to -th. Because of the obvious resemblance to a Greek locative, it is generally assumed that the Etruscan form was borrowed from Greek. But a little reflection will suffice to show that this is quite improbable. Everyone agrees that the Etruscan alphabet was adapted from a Western Greek alphabet, apparently adopted from Greek merchants, and that the Etruscans borrowed a number of words from the Greek language. But it is clear that the influence of Greek civilization was actually superficial; the inner spirit of Etruscan civilization remained untouched by Greek rationalism. This is clear, for example, from their respective artistic and religious expressions. Consequently, there is no reason to assume that the influence of Greek culture was so pervasive as to affect the declension of nouns. It is much more likely that the -thi was an alternative locative form preserved by parallel descent from the common ancestor of Etruscan and IE, vestigially preserved in Greek but generalized in Etruscan.

The same applies in regard to several items of vocabulary. The noun tin or tins, "day" or "Jupiter", resembles an IE word which appears in Baltic and Slavic. It is apparently a genuine Etruscan word since it has its genitive in the archaic -n, and also because any similar word seems to be lacking in the surrounding languages.

The noun *laut-, with archaic genitive in -n, lautn or lautun, "family" or "clan", resembles an IE word which appears in German as Leute and in English as lewd. Pfiffig attributes it to borrowing from nearby IE languages, but I regard it as genuinely Etruscan because of its archaic genitive.

The verb tur-, "to give", seems to be an -r extension of a root *tu- which is widespread throughout the Nostratic realm. It appears in IE and Uralic with a laryngeal or velar extension, and in Eskimo with an -n extension.

The verb ces- (kes-), "to lie, to be recumbent", seems to be an -s extension of a root which is well-known in IE. A similar word ki- or kui- is widespread in FinnoUgric languages also (see 1.3, 1977, p 105). This is not included in Collinder's Uralic list because it does not seem to be attested in Samoyedic languages; therefore it is excluded from Collinder's list of possible IndoUralic cognates. It should always be borne in mind that Collinder's listing of IndoUralic cognates is conservative because of his insistence on representation in Samoyedic as well as FinnoUgric.

The noun tul, "stone", is similar to a Pan-Altaic word, though apparently missing in IE and Uralic. This is not surprising once we recognize the Nostratic origin of Etruscan. As in any far-flung linguistic family (or superfamily), some words will be retained in outlying members yet be lost in intervening divisions. Another example is IE *pet'- (*ped-), "foot", which appears in Altaic but not in Uralic. In fact, Uralic seems to be remarkable for a number of omissions of this kind. Perhaps the precursor of Common Uralic went through a period of rapid replacement of vocabulary due to taboo; perhaps this explains some of the apparent paucity of IndoUralic and UralAltaic cognates.

Etruscan had two enclitic connectives, -m and -c (-k), used much like Latin -que, Greek -te and Sanskrit -ca. Pfiffig states that the -c form was late, not being used until the 5th century BC. Accordingly, he attributes it to borrowing from Italic, and in support he cites Latvian "un", Finnish "ja" and Turkish "ve", borrowed from German, Gothic and Arabic, respectively. But there are a couple of things wrong with his assumption. First, the supporting examples which he cites are not actually relevant, being separable conjunctions instead of enclitics. Second, the form could be missing from the earlier inscriptions by accident. Third, the form could have been missing for stylistic reasons, perhaps being so "common" in popular speech as to be considered unsuitable for use in solemn inscriptions, becoming acceptable only later. Fourth, it could not have been borrowed from Oscan or Umbrian, for in these languages the enclitic had become -pe by this time. Fifth, it could hardly have been borrowed from Latin or Faliscan since the speakers of these languages were peripherally located with respect to the main body of Etruscan speakers, and at this stage their influence on Etruscan was slight; the Etruscans were still the dominant cultural power. The same applies in regard to any supposed influence from Venetic. Sixth, the form is not uniquely Indo-European since it has been verified for Uralic as well, occurring in both branches of that family. Consequently, there is no reason why it should not be regarded as due to parallel descent from Nostratic, despite its apparently late appearance in the inscriptions. In view of the difficulties listed above, this is a far better suggestion.

In fact, there is good reason to believe that Etruscan and IE evolved in mutual isolation until the latest phase of their respective histories. Despite the close similarity in the pronouns (as available) and in the singular of the nominal declension, there are some details of the verbal morphology which seem to have been independently evolved. There is, for example, a general lack of personal endings in the Etruscan verb, and it is hard to see how the tense and mode signs would correspond to those in IE; of course, it has to be admitted that the nature of the evidence prevents us from knowing much about the verbal morphology. And the verb to be was am-, not es- as in IE.

Also the plural of nouns was -r, not -s.

Again, the words for family relationships are mostly different. An exception is nefts, uncertain whether "nephew" or "grandson", which may be a borrowing from Italic, as claimed. I suspect, however, that the real meaning is quite uncertain, the supposed meaning having been suggested by an accidental resemblance of sound. In any case, the point is not particularly significant; the names for family members can be quite volatile in most languages. The real point is that the evolution of these names was mostly independent in the two languages.

10. Special Problems

Perhaps the most striking indication of the early independence of Etruscan is the fact that the numerals from 1 to 10 are entirely different from those in IE -- obviously, this is no argument against the ultimate identity of the two languages. In the past, IndoEuropeanists have mistakenly assumed that numerals are among the most stable elements of vocabulary, but this is due merely to the limitations of the intellectual experience of these scholars. In other families the numerals can be quite variable. And even in IndoEuropean itself there is considerable variation. In Anatolian, for example, the words for 1 and 4 are aasma and meu or mau, entirely different from anything in IE-Proper; the relative uniformity throughout most of IE-Proper is due to the focal nature of its sudden expansion from a geographically restricted dialect area. And even in IE-Proper there was great variation in the words for 1. The main point is that the evolution of the numerals occurred independently in Etruscan and IE, underscoring their mutual isolation from the earliest date of their existence in the Balkans.

I draw the following conclusions:

1. Thanks to the impressive achievement of the modern Etruscologists, led by the great Pallottino, we can now understand Etruscan with far greater confidence than before.

2. Because of the funereal and religious nature of the evidence, our understanding of the pronominology and the verbal morphology, and of the meaning of many words, will remain substantially less complete and less certain than would otherwise be possible.

3. Nevertheless, the evidence, and our understanding of it, are quite enough to enable us to state, with full confidence, that Etruscan was undeniably a typical member of Nostratic. Once we realize this, a great deal of the mystery behind the obvious fundamental similarity between Etruscan and IE disappears. It is no longer necessary to attribute it to wholesale borrowing on the part of Etruscan, which was unrealistic to begin with.

4. Primordially, Etruscan was most closely related to IE and more distantly to Uralic, which is exactly what we should expect on the basis of prehistoric areal dialectology, whereby languages of a given family (or superfamily) tend to most closely resemble their nearest geographical neighbors except where the pattern has been disturbed by secondary expansions. This is particularly clear in the development of the genitive, shared only with IE.

5. It is largely a matter of intellectual taste, or academic prejudice, as to whether IE and Etruscan ought to be grouped into a single, more inclusive, family. Logically, they should be, for their parallel occurrence in the Balkans represents the results of an apparently unique immigration from the East. Their common origin goes back to at least as early as 6500 BC.

6. The great divergence between Etruscan and IE, and the much smaller divergence between Anatolian and IE-Proper, are consistent with the heterogeneous archeological panorama in the Balkans for the early Neolithic. Both phenomena suggest the early establishment of many parochial communities, presumably nearly uniform in language initially, covering most of the Balkan region. Apparently there was not enough communication among these communities to inhibit a swift divergence of dialects.

7. Only three of these dialects ever became known to history. These were the precursors of IE-Proper, of Anatolian, and of Etruscan. Early IE-Proper was spoken at the Northern fringe near the upper bend of the Danube in Hungary; it was here that the explosive Linear Pottery expansion had its focus. Early Anatolian was spoken a little farther to the South, not close enough to be swept up in the Linear Pottery expansion. In contrast, early Etruscan was spoken much farther away at the Western or Southwestern fringe, which accounts for its greater isolation and divergence.

By historical times this scene had greatly changed. The details will be discussed in the next chapter.

11. OUTLINE PREHISTORY

It is now clear that the prehistory of the IndoEuropean family of languages and its speakers cannot be adequately appreciated except in concert with several other linguistic families of the same origin, all having evolved from a single earlier language which we call Nostratic. This realization is important because it enables us to shed some light on phenomena which otherwise would resist interpretation. Thanks to the link with Uralic, we can now understand the origin of gender in Indo-European, and because of the more distant link with EskimoAleut we can understand the relict nature of the neuter r-n declension. We can also resolve the paradox presented by Etruscan, which with its peculiar names for family members and for numerals shows clear evidence of long continued development in effective isolation, yet also exhibits similarities of a fundamental nature with IndoEuropean, Uralic and even Altaic.

Of course the concept of Nostratic assumes the onetime existence of a specific group of people who lived in a specific territory. and who spoke mutually intelligible dialects of a single language which was ancestral to all the linguistic families which we now recognize as members of Nostratic, for without that assumption there would be no realistic explanation for the far-flung similarities which we observe. This means that we have to make some effort to suggest a time and place where these people probably lived, and to make some assumptions about the circumstances of their lives.

Because of its central geographical position, I assume that Central Asia was the general region where these people probably lived, perhaps a little to the Southeast of the Southern Urals, between the Aral Sea and Lake Balkash. Obviously, this is no more than a wild guess.

The time is likewise uncertain. I imagine that it may have roughly coincided with the transition from the Late Upper Paleolithic to the Mesolithic, about the time when the most recent Ice Age began to wane. In fact, I suspect that the language had already been fragmenting in a general and slow non-focal expansion in several directions -- like the diffusion of molecules in a gas, or like the spreading of a colony of bacteria in a culture dish, or like the enlarging of a fairy ring of toadstools in a meadow. Such diffusion would be a natural consequence of the central position of these people. Of course, this group was only one of a number of groups of people who were involved in independent expansions simultaneously, such as the expanding group in East Asia speaking languages related to Sinitic.

It is almost idle to speculate as to the "race" of these people. Whenever we get any reliable information on the racial background of a group of people, especially of the great history making groups, we practically always find that they were of very heterogeneous origin. This is true of all the historically known nations, and there is certainly no reason to believe that there was any difference in prehistoric times. In fact, one of the major strengths of humanity as a species is that we have all the genetic purity of alley cats.

If pressed, however, I would suggest that the earliest speakers of anything which we might call "Nostratic" probably were Caucasoids of two principal subracial types, perhaps mainly what anthropologists used to call East Mediterranean mixed with a so-called Alpine type similar to that which now dominates in Central Europe -- in fact, I suspect that a relatively high incidence of the blood type B, a feature of both these subracial types and now widespread in Asia and in Eastern and Central Europe, was broadly dispersed by the expansions of Nostratic speaking populations. It is to be expected that these people were constantly mixing with the populations which they encountered. I believe that the earliest IndoEuropeans, in particular, consisted mainly of these two types.

The expansion of the Nostratic speakers, especially to the West, was probably facilitated by the recession of the glaciers at the end of the Ice Age. Late Upper Paleolithic hunters of Nordic and other types, long preadapted to a near-Arctic climate in Central and Eastern Europe, were now able to occupy the Northern parts of Europe, including Britain, while still continuing their old way of life. Meanwhile their old territories had now become attractive to people of more Southern and Eastern origin, people of now Mesolithic culture who also continued a hunting and food gathering economy.

Another incentive to Nostratic expansion at this time may have been an increasing dessication in Central Asia due to the advent of the Neothermal conditions which we observe today.

A third incentive may have been the introduction of Neolithic economies and techniques from the Fertile Crescent. It may be true, as Bomhard's results seem to imply, that the precursors of the IndoEuropeans and the AfroAsiatics were still united at the Southwestern fringe of Central Asia at this time and that they became acculturated to the Neolithic way of life as a still united people. If so, it may have facilitated the later expansions of these people, the Pre-IndoEuropeans Westward along the North shore of the Black Sea into the Balkans, and the Pre-AfroAsiatics Southwestward into Mesopotamia. This may have been around 7000 BC or a little earlier. It is important to remember that the acculturation of the Uralic people was a separate and probably later development.

The Westward penetration of the Pre-IndoEuropeans may well have been focal in nature, almost explosive in its swiftness.

For if it is true that they had become acculturated to the Neolithic way of life, and if the people they encountered were still in the Mesolithic phase as I believe they were, they would have had little opposition -- Childe's insight in regard to the revolutionary effect of Neolithic acculturations was correct. It is probably true that the initial Neolithic development in Palestine and the Fertile Crescent was an evolutionary outcome of the previous Mesolithic conditions in this region, but once the new way of life had begun to penetrate into other areas, the immensely greater productivity and reliability of the new techniques were overwhelming. The swift occupation of extensive regions by agriculturists and their concomitant languages was inevitable.

In contrast, in their penetration through the Fertile Crescent and into Northern Mesopotamia the Pre-AfroAsiatics found themselves surrounded by peoples who had long since been practicing Neolithic techniques. Consequently their new neighbors were able to provide a more effective resistance against people who were in essentially the same technological condition as themselves. This must have slowed the Southward progress of the AfroAsiatics quite substantially, which may account for their apparently late emergence in their historically known positions, including North Africa. In fact, the wonder is that they were not absorbed. Indeed, they did not escape linguistically unscathed, for theirs was the only Nostratic language which suffered such an early and drastic syntactic and morphologic revolution.

If the foregoing scenario is correct, it seems to explain why IndoEuropean, together with Etruscan, retained so much greater similarity to Uralic in regard to syntax, morphology and pronominology.

In following the special history of the IndoEuropeans, reference will be made to the 4 maps herein. These show an outline sketch of continental Europe together with the Black Sea and the Northern half of Anatolia, featuring most of the major rivers. The maps were drawn with a flat-bed plotter driven by a home computer. Lat-long data were taken from several maps in the National Geographic Atlas of the World, 4th edition. These data were transformed so as to be projected perpendicularly against a tangent plane located at a point near Budapest. To save time and personal energy, many coves and river bends were "straightened" and innumerable offshore islands were allowed to "sink" without compunction. Even Britain, the land of my ancestors, disappeared without a gurgle. With the Upper Bend of the Danube as reference, the Great Circle distances to several points are about as follows:

Mouth of the Danube	*825 KM*	*or*	*510 Miles*
Source of the Danube	*775 KM*	*or*	*480 Miles*
Mouth of the Rhine	*1175 KM*	*or*	*730 Miles*
Mouth of the Elbe	*1000 KM*	*or*	*620 Miles*
Mouth of the Oder	*770 KM*	*or*	*475 Miles*
Mouth of the Vistula	*735 KM*	*or*	*455 Miles*
Source of the Dniester	*345 KM*	*or*	*215 Miles*

When the precursors of the IndoEuropeans and Etruscans settled in the Balkans, apparently about 6500 BC, they seem to have immediately begun to diversify into a number of autonomous communities. This is suggested by both the emergent linguistic phenomena and the heterogeneous StarchevoKoeroes archeological panorama. The Pre-Etruscans remained effectively isolated in the Western Balkans, as suggested on Map I, their language evolving independently from IndoEuropean. Meanwhile IndoEuropean was evolving in several dialects in the Northern part of the Balkans, as shown on the map. Such was the situation down to about 5200 BC.

At this time some of the Northern IndoEuropeans -- those near the upper bend of the Danube, near Budapest -- became aware of the fact that their cereals had now become acclimated to a colder climate. They immediately began expanding their settlements into the Northern half of continental Europe, spreading in a fan-like pattern in several directions simultaneously, introducing the Neolithic way of life into these regions. The resulting archeological phenomenon was given the name Danubian I by Childe because of its origin on the upper Danube and because it was the earliest Neolithic culture to emerge in this territory. However it is called the Linear Pottery culture by most archeologists from the appearance of the lines incised on the characteristic pottery, and this designation has been shortened by Milisauskas to simply the Linear culture. As indicated on Map II, I have identified this culture with the IE-Proper dialect of IndoEuropean.

This expansion was remarkable in several ways. First, it was exceptionally rapid, by far the most rapid of all the early Neolithic expansions in Europe -- with the possible exception of the initial penetration of the Pre-IndoEuropeans-Etruscans into the Balkans in the 7-th millennium. According to the dates provided by Milisauskas (radio carbon as corrected by tree ring data), the Linear Pottery culture first appeared near the upper bend of the Danube at roughly 5200 BC, and in only 200 years had spread through most of the Northern half of continental Europe from the Netherlands to Eastern Poland, and had extended

Southeastward to the upper Dniester in the Western Ukraine in another two centuries.

Second, the stylistic features of this culture were remarkably uniform throughout the expansion territory, from the Netherlands to the Ukraine and from the Danube almost to the Baltic Sea in the North. This was a consequence of the rapid expansion, for after a few centuries the styles in the center of this territory began to diversify into several new genres. Also, it implies that the people involved in this expansion came from a narrowly restricted geographical area, for otherwise such a widespread uniformity would have been impossible. It also suggests that the language they spoke consisted of a single dialect which initially was highly unified. This is because the identity of a language is a cultural phenomenon, and uniformity of language would be consistent with the uniformity of other cultural features, including those archeologically preserved. For this and other reasons, I have identified this dialect with undivided IE-Proper since all the historically known branches of Indo-European, except Anatolian, can be derived from a single dialectal form of the language, and also because the timing is appropriate from a glottochronological standpoint. The remarkable heterogeneity of these branches which we observe in historical times is due merely to the fact that this expansion occurred at an earlier date than is ordinarily realized. In short, this was a classic example of a focal expansion.

Third, the stylistic features of the Linear culture were startlingly different from those of the presumably parent StarchevoKoeroes cultures in the South, although there is some indication that the adjacent cultures were transitional. In general these features are more stark, less elaborated, hence more in keeping with the less confining environment of the North; this was a period when the IE-Proper people were moving with little opposition into regions which were underpopulated by Neolithic standards, which accounts for the rapid expansion. Further, the houses were larger, and both they and the villages which contained them were more widely separated; there also was a general lack of the clay figurines found in the Southern cultures. I get the impression that this development may have originated in an ideological revolution on the part of a dissident minority at the Northern fringe of the old settlement area. Perhaps an analogy can be found in the Protestant Reformation with its simplified and stark religiosity and its relatively aggressive involvement in scientific and industrial developments.

Fourth, the Linear culture was apparently a riparian civilization in that the rivers seem to have been a major factor in its rapid dissemination and in the siting of the settlements, which have been found mainly on loess soils near rivers and minor streams. The existence of these soils was due to the depo-

sition of windborne material which had been produced by the grinding of rocks in the movement and recession of glaciers during the Ice Age and its waning; the deposition was greatest in lowlying areas, which naturally included the valleys near the streams. This was a part of a constellation of circumstances which contributed to the rapid deployment of these settlements. The loess soil was fertile and easily worked by the primitive techniques of those days, which may have involved slash and burn operations; this made it attractive for settlement. Trees and shrubbery were available for the building of houses and for the sheltering and feeding of animals, and perhaps even the building of rafts or boats for transportation along the nearby streams, although these vehicles are not apparent in the archeological record. Nevertheless, it seems plausible that such a means of transportation was used, for the spotty distribution of the settlements suggests that when the soil near a village became exhausted some of its population often moved with their belongings to a new and distant location farther along the stream, ignoring earlier settlements in the intervening territory. This would lead to the establishment of initially disconnected settlements distributed like the holes in a Swiss cheese; such a leapfrogging deployment seems a natural implication of the archeological phenomena since it would have been conducive to the observed swift occupation over extensive regions. Of course in many cases waterborne transportation was not needed since an overland trek along a stream would often be feasible. It is like the use of railroads and wagon trains in the rapid deployment of settlers and their General American dialect of English across the Northern half of the United States during the latter half of the 19th century.

Fifth, the Linear Pottery culture was remarkable for its absence of any indication of organized warfare -- anything resembling fortifications, for example. This is consistent with the rapid deployment and the initially spotty siting of the settlements, which suggests that any opposition on the part of the native Mesolithic population was negligible. It is also consistent with the almost complete lack of common words for warfare in the separate branches of IE-Proper, which suggests that the common view of the undivided IndoEuropeans as an egregiously warlike people is without foundation. I get the impression that the superior productivity and reliability of the Neolithic way of life was so obvious to the sparse Mesolithic population that they quickly adopted the language and the Neolithic techniques and willingly joined in the expansion. If this is correct, it suggests that the Nordic element in the Neolithic population was steadily increasing as these people penetrated Northward; this is plausible because it is in fact the distribution of subracial types that we observe in Europe today. There is an interesting *parallel in the Eastern part of the Nostratic domain, where the Mongoloid racial types become increasingly prominent as we go Eastward through the Uralic and Altaic populations. The Eastern Uralians (i.e., the Samoyeds), though essentially Caucasoid, are transitional.*

All this was a consequence of the physical structure of the European continent, which because of Northward tectonic pressure from Africa has crinkled into mountain ranges from the Pyrenees and the Alps to the Carpathians. In consequence, there is an East-West continental divide extending from the Northern Alps through Czechoslovakia Eastward to the Northern Carpathians in Rumania, and from this divide the major rivers radiate like the spokes of a wheel. North of the divide, the drainage is mainly via the Rhine, the Weser and the Elbe, which empty into the English Channel and the North Sea, and also the Oder and the Vistula which empty into the Baltic; an Eastern tributary of the upper Vistula is the San (not titled on the maps), whose source is close to that of the Dniester, which drains the Northeastern flank of the Carpathians and empties into the Northwest corner of the Black Sea. On the South, the drainage is mainly by many Southward flowing rivers (not marked on the maps) emptying into the Danube, which rises in the Northern Alps near the source of the Rhine and runs Eastward into Hungary then Southward into Yugoslavia and again Eastward to the Western shore of the Black Sea. Beginning in Hungary and Austria, the Linear settlers followed the rivers Northward to their sources near gaps in the continental divide. From these, the settlers moved beyond the divide to the sources of other rivers which flow in several Northerly directions as above described. Following these rivers, the settlers were led inevitably into the fan-like deployment which soon covered most of the Northern half of continental Europe.

By about 4800 BC the first flush of the almost explosive IE-Proper expansion had been completed. Events were now entering a new phase. By 4600 the archeological phenomena begin to indicate the emergence of several new stylistic traditions, both within the old Linear territory and at various points along its periphery, marking the rise of the Middle Neolithic in these regions. It was a time at once of consolidation, of parochial differentiation, and of continued expansion; it lasted roughly 1500 years, beginning and ending earliest in Central Europe and later at the peripheral locations. Archeologists have given such names as Lengyel, Roessen, Michelsberg and Aichbuehl to various new traditions emerging in and around the central region -- Czechoslovakia and Hungary, and the Southern parts of East Germany and Poland. The major traditions emerging at the periph-

ery of the old Linear territory were those called Tripolye and Funnel Beaker in addition to others resulting from Westward penetrations into Switzerland and perhaps Britain (Windmill Hill), both of which had previously been acculturated to the Western Neolithic tradition with its presumably alien language.

Under the circumstances, it is highly probable that the Middle Neolithic period was also a time of linguistic diversification with new dialects emerging out of the IE-Proper background, and with continued divergence of previously existing dialects such as Pre-Anatolian and Pre-Etruscan and many others which never became known to history. On Map III I have indicated what I believe to have been the likely positions of early dialectal associations as they existed at the beginning of this period. Noteworthy is an early association that included the precursors of Germanic, Baltic and Slavic at the headwaters of the Oder and the Vistula, and also of Celtic and Italic near the upper bend of the Danube. There was also a string of dialects which included the precursors of Slavic, IndoIranian, Albanian-Armenian and Greek along a line which runs from the Upper Vistula and the San to the Upper Dniester -- I call this the **Pre-Satem Line** since it included all the dialects (except Baltic) which later developed the characteristic Satem features. Early positions of Pre-Tocharian and Pre-Anatolian should also be noted. Special notice should be taken of the region between the Rhine and the Elbe (including the Weser valley, not marked), which earlier had been densely occupied by Linear settlers, and which now was populated by people who probably spoke IE-Proper dialects which never became known to history. Some of these dialects are indicated on the map by the symbol UD.

Funnel Beaker is the name given by Milisauskas to the culture which Childe called First Northern and which Piggott called TRB, short for Trichterbecher, the name given by German scholars from the characteristic shape of the pottery. It resulted from a conversion of the local Mesolithic peoples to the Neolithic way of life which had been introduced by the Northwestward advance of Linear Pottery settlements down the Oder. It developed earliest on the lower Oder and in Denmark, then spread Eastward to cover the Northeastern third of Germany and also Poland. It provides a convenient setting for the early dialectal association which included the precursors of Germanic, Baltic and Slavic. It is reasonable to suggest that the peculiar change of the case stem -bh- to -m- in these branches occurred at the beginning of this period, about 4800 BC. Needless to say, the separation of Germanic and the development of Satem phenomena in Baltic and Slavic were later events.

In a similar manner, the Tripolye culture resulted from a Southeastward extension of Linear Pottery settlements on the up-

per Dniester. It appeared first as the Pre-Cucuteni culture on the middle Dniester and spread Northeastward to the Dnieper and Southwestward into Eastern Rumania. The Tripolye culture is especially interesting for the following reasons:

1. One of the dialects spoken by the people of this culture may have been ancestral to Tocharian. This is plausible because Tocharian was a Centum language which represented an early Pre-Satem offshoot of Eastern IE-Proper. Primordially, it was most closely related to Pre-Greek and especially to Pre-Albanian-Armenian, which later became Satemized. This is consistent with the apparent origin of this culture in the Linear settlements on the upper Dniester.

2. The people of the Tripolye culture seem to have been the first to domesticate any horse-like creature; they may perhaps be identified with Gimbutas' famous Kurgan people. This development may have occurred as early as the latter half of the 5th millennium as Gimbutas has suggested, but Milisauskas cites Boekoenyi as placing the earliest domesticated horses at the Tripolyean B settlement at Dereivka on the Dnieper in the Southern Ukraine at about 3600 BC. In any case, the Kurgan people were too late, and too parochial, for identification with the undivided IndoEuropeans or even the undivided IE-Proper people. Nevertheless, it seems plausible that they spoke "Indo-European" dialects, presumably related to an early version of Tocharian.

In general, besides expanding into new territory in several directions as above described, -- Westward into Switzerland and Britain, Northwestward into Denmark and Southern Sweden, Northeastward into Byelorussia, and Southeastward into Southern Russia -- the Middle Neolithic successors of the Linear Pottery culture show evidence of an inner intensification of settlement patterns and consequent social stress. Within the old Linear territory the best land had already been taken, so the descendants of the old Linear population were impelled to adopt more intensive farming and herding practices and to occupy less desirable land in the old Linear domain as well as to emigrate into new territory, both Mesolithic and Neolithic. In short, the entire domain became a diffuse center of continued expansion, both inward and outward, which now was non-focal in character. Apparent evidence of social stress can be seen in what seem to be the remains of fortifications dating from this period.

Some archeologists regard these structures as cattle kraals, enclosures used for restraining cattle and other domestic animals. But while I have no doubt that this was one of the uses, I believe the major purpose was to protect both animals and people from attacks from competing tribes, for these structures are not the only apparent indications of deliberate warfare -- weapons are distinctly more prominent than previously. The reader is referred to Milisauskas on this point (43.1, pp 177-179).

Consonant with this, it is very likely that there was a steady rise in the social importance of warlike chiefs and their violent henchmen during the Middle Neolithic -- roughly 4500 to 3000 BC, depending on the location. In order for organized warfare to exist it is necessary for someone to do the organizing, and such individuals usually appear in times of social stress. Unfortunately, such times are the normal state of affairs in civilizations where the density of population and the need for gainful employment have caught up with the availability of resources as currently perceived and with the productivity of the prevailing technologies as currently accepted and utilized, and this is most of the time in most societies.

Even during the halcyon days of the Linear expansion the potentiality for organized warfare was already latent, for potential booty in the form of cattle and preharvested grain and field slaves was rife. It was only the ready availability of easily worked and highly productive land, together with an initially low population density, that made organized warfare seem untempting and unnecessary. Yet even during the Linear period there was a need for leaders to organize the siting of houses and villages, even if at no higher than a family or clan level. Thus there was occasion for the use of such a word as *reek'- (*reeg-) to designate an individual who was expected to take the lead in various kinds of undertakings, originally usually peaceful.

It would be irresponsible to leave a discussion of the Middle Neolithic without making some mention of the truly astonishing Megalithic phenomenon, although with one major exception it was essentially peripheral with respect to Post-IE-Proper developments. That exception is the Northwestern part of the Funnel Beaker domain -- North Germany, Denmark and Southwestern Sweden -- territory which in historical times has been solidly occupied by speakers of Germanic dialects. A clear presentation of much of the territory eventually covered by Megalithic monuments and tombs was provided by Piggott (see 21.1, p 61, Fig 29). In addition to the above, it eventually covered the Northeastern tip of Scotland, Ireland, the Western fringe of Britain and Scotland, most of France including especially Brittany in the Northwest and Provence in the Southeast, the Pyrenees, Portugal, Southern Spain, Catalonia in Northeastern Spain, some Mediterranean islands like Minorca, Malta, Corsica, Sardinia and Sicily, and the heel of the Italian "boot".

Childe used to think that the Megalithic phenomenon was inspired by contact with the civilizations of the Near East and the Eastern Mediterranean, including Mycenae. But I was always skeptical of this since I could never see much resemblance in either form or style. And by taking into account the tree ring correction of radio carbon data, Colin Renfrew has convincingly shown that the earliest appearance of the phenomenon (in Brittany) clearly antedated the rise of the great civilizations of the Eastern Mediterranean. So it is now clear that it was a "home grown" development associated with the Southwestern, Western and Northwestern fringes of Europe. It is true that there were also other quasi-Magalithic phenomena in India, Japan, Palestine, the Caucasus and other places, but with the possible exception of Palestine these were so distant and widely separated that they seem to have been unrelated to the phenomenon under discussion.

The distribution of the Megaliths is obviously remarkable, both regarding where they occurred and where they did **not** occur. For example, why did they cover the Southwestern fringe of Sweden but not the interior? Why did they cover the Northeastern tip and the Western fringe of Scotland but not the interior? Why did they cover Western and Southwestern Britain but not the East and the North? Why did they cover the Northern, Western and Southern parts of Iberia but not the East and the interior? Why did they cover a number of Western Mediterranean islands and Sicily and the heel of the Italian "boot" but not the interior of Italy? In general, the phenomenon is most closely associated with islands and the shores of continental Europe, penetrations inland being apparently secondary.

It seems to me that there is only one way to account for the seemingly capricious distribution of this phenomenon, and that is to assume that there must have been persistent seaborne migrations and commerce between the Western Mediterranean and Scandinavia via the Atlantic Ocean, using Portugal, Brittany and Ireland (with adjacent parts of Britain and Scotland) as the major waystations, and that the **idea** of erecting prestigious Megalithic monuments and tombs was instigated, spread and maintained for some 3000 years by internationally based merchants who maintained this commerce, which probably varied in intensity from age to age.

In fact, this could well have been the major factor in the deployment of Neolithic settlers of West Mediterranean subracial type, whose apparent descendants are still prevalent in precisely those areas covered by the Megaliths (except Scandinavia).

But if all this was going on, why can we not read about it in our history books? Well, there are a couple of reasons. For one thing, no history in the Classical sense was being written in those days, so far as we know. For another, such history as we have was written by Greek and Roman intellectuals who had little knowledge of commercial activities; besides, Roman sea-going commerce was mainly in the hands of foreigners, who had a vested interest in not divulging their sources. In addition, by Classical times the Megalithic activity was largely over. But this does not mean that Atlantic commerce had ceased, for in his Commentaries on the Gallic Wars Caesar describes a major naval battle against an Atlantic fleet, based in ports on the coast of Brittany, which was quite capable of of being sailed in regular ocean traffic.

Those who believe that the people of the 4th millennium BC were not capable of sailing the Atlantic (as well as the Mediterranean) are misinformed. How else do they imagine that settlers with Neolithic culture and domesticated plants and animals were imparted to the British Isles and the Canary Islands? To imagine that they had no boats more substantial than coracles is quite fallacious. Such a notion can be sustained only by our general historical ignorance of sea-going activities in prehistoric times; it involves the fundamental fallacy of assuming that technological sophistication necessarily increases monotonically in all circumstances.

As for the Mediterranean, we know that by the 7th millennium Neolithic settlers were already occupying a number of islands with their impedimenta, and sea-borne traffic is the only way they could have been transported. In fact, it is probable that they were using these islands as secondary bases for further expansion, for this is suggested by the scattered coastal distribution of the archeological remains of the Cardial culture in the Central and Western Mediterranean. It should be borne in mind that the Mediterranean can be quite rough when storms arise. People who were capable of dealing with the Mediterranean would have had little difficulty in adapting their boats and techniques to Atlantic conditions during the intervening 2 millennia once they became aware of the possibilities.

How did the Scandinavians get into the act? Perhaps this can be understood when we consider the origin of the Funnel Beaker culture. This appeared earliest on the lower Oder, where it seems to have originated in an intimate merger between the Mesolithic shore-based Erteboelle culture and the local version of the Linear culture, which had been advancing Northwestward down the Oder. In view of their "Kitchen Middens", the people of the Erteboelle culture seem to have been well supplied with sea food over a long period. Although they evidently adopted the language and the Neolithic impedimenta of the local Linear immigrants, they may well have retained an intimate connection with the sea. In later centuries they might easily have extend-

ed their excursions to Northeastern Scotland, Ireland, Southwestern Britain, Brittany, and perhaps to Portugal and into the Mediterranean. In doing so, they may have mixed with sea-going people of West Mediterranean subracial type, perhaps related to the Basques. When some of them returned to Scandinavia, they may well have imported the **idea** *of erecting Megalithic monuments and tombs. In fact, there is no reason why there could not have been several "Viking Ages" during at least 4 millennia, culminating in the presence of the Normans in Normandy, Britain and Sicily in the Middle Ages. In this connection, it should be noted that the emergence of the historical Vikings was due not so much to improvements in naval craft as to their rather sudden realization of the wealth and political weakness of the rest of Europe, which was vulnerable to predatory operators who had the freedom to choose the time and place of attack; when this weakness began to be rectified in the High Middle Ages the Viking Age was soon over.*

I believe that Megaliths were first erected as isolated stones intended as landmarks for the convenience of sailors. Such people would naturally be aware of the usefulness of certain conspicuous celestial objects for indicating time and direction, and if we assume that peasant villagers located near the sea sometimes "doubled" as sailors as described by Hesiod in his "Works and Days", in course of time it would be realized that stones erected on land could be aligned so as to aid in analyzing the movements of such objects for their convenience in regulating the seasonal activities of people on land. As the centuries passed, the usefulness of such imposing structures for striking awe into the people of other villages farther inland could easily have led to their use for community prestige and cultic practices in the hinterlands.

Of all the Megaliths, the most magnificent in its sophistication was Stonehenge. Like the music of Bach in relation to the Baroque period, it set the cap on the Megalithic Age. It was constructed during a period of some 1200 years, from about 2700 to 1500 BC, which was about the time when most Megalithic construction ceased.

Every dog has its day, and every day has an end. By 1500 egoistic Bronze Age aristocrats and their violent supporters from Central Europe were penetrating into the old Megalithic territories and dominating the earlier Neolithic establishments. The community spirit which had sustained the Megalithic constructions became a thing of the past. It is most regrettable; change is not always for the better.

The Late Neolithic is largely the province of the 3rd millennium; it culminated in the rise of the Central European Bronze Age, roughly 2000 BC, generally earlier in the South and later in the North. Logically, the division between Middle and Late Neolithic is rather arbitrary since the developments of the Late Neolithic were essentially a continuation and intensification of those of the earlier period. The division is mainly for the convenience of archeologists, who recognize several new styles in weapons (e.g., stone "battle axes") and in pottery. As in the former period, the regions where these styles prevailed tended to overlap to some degree.

There has been a tendency for some archeologists, particularly Gimbutas, to claim that Central and Northern Europe were invaded, or at least "infiltrated", by pastoralists from Russia or Central Asia during this period or perhaps a little earlier, imparting dialects of IndoEuropean to these regions where earlier it had supposedly been unknown. But such a claim has nothing to recommend it. My reasons for rejecting it are given in an earlier chapter and need not be repeated here. These scholars have overlooked the fact that pastoralism was an essential feature of all the Neolithic cultures since their first appearance. This specifically includes the Linear Pottery culture, where male cattle were often castrated. Also, as I have stated earlier, the apparent fact that these people did not have domesticated horses is quite irrelevant.

As a matter of fact, in historical times warlike horse-riding pastoralists invading from the steppes have had remarkably little effect on the linguistic patterns in non-Russian Europe. The only marked exception is the Hungarians. All the others, from the Scythians to the Huns and the Mongols and Tatars, have left the patterns essentially undisturbed over the long view.

During the Late Neolithic there was a general phasing out of the older genres and a replacement by newly emerging pottery styles. Milisauskas mentions such phenomena as "the southeastern European Vincha-Plochnik, Late Gumelnitsa, Salcutsa, and the Late Tripolye culture of the Ukraine, the central European Corded Ware, Globular Amphora, Bodrogkeresztur, Baden or Channelled Ware (Figures 7.1 and 7.2), late Funnel Beaker, and Bell Beaker cultures." (see 43.1, pp 181 - 189). As stated above, there was a tendency for these cultural territories to overlap to a considerable degree, suggesting that in most cases there was only a minor displacement of populations and associated languages; nevertheless, the fact that these genres do overlap suggests that there was a persistent interchange of prominent individuals and their supporters from one cultural area to another, often far afield. Bell Beaker remains, in particular, are found sporadically over an immense territory -- in Sicily, Sardinia, Spain, France, more densely in Poland and Germany, and especially in Britain and Scotland; it appears very much as if some of these people took advantage of pre-existing *facilities for sea-borne transportation. This is not surprising in view of the fact that they were still in the Megalithic era.*

The Bronze Age began about 2000 BC in Central Europe, earlier in the South and later in the North. In general, we find a still further acceleration in the trends already operating since 4500 BC. These include at once a marked increase in technological sophistication, in parochialization of culture despite evidence of mixing, in evidence of internecine warfare and the prominence of warlike chiefs, and in the continued centrifugal movements of peoples based on Central Europe as a general center of expansion. These movements may have been instigated, in part, by a constant desire for relief from the caldron of warlike activity in Central Europe; tribes tended to migrate away from this general center, and outlying tribes were impelled still farther outward.

In fact, we know by historical remains that the Anatolian speakers had been resident in Anatolia by 2000 BC for some time, for this originally unified branch had already differentiated into separate languages. But they were not alone, for the Greeks were right behind them in the Southern Balkans and Greece. This may also have been about the time when the IndoIranians began their tremendous expansion into Southern Russia and eventually into the Caucasus, Iran and Northern India. All these were early Bronze Age phenomena.

The time when the Celts began their Westward expansions is uncertain. They may well have begun in the Bronze Age, about 1500 BC, but in any case we are confident of their expansions during the Halstatt and La Tene Iron Ages in later centuries. From circumstantial considerations, I strongly suspect that the Celts were not the first speakers of Post-IE languages to enter the British Isles.

On Map IV I have marked what I believe to be the approximate migration routes of the speakers of some of the historically known branches of Post-IE. It is likely that there was a considerable Nordic element in most of these populations, especially in the North, due to an earlier incorporation of Northern Mesolithic people into Neolithic societies of IE origin. The display is anachronistic in that I have made no attempt to indicate the dates when the migrations began.

This is about as far forward as I care to bring this narrative. Actually, through most of this discussion I have been treating not IndoEuropeans, but Post-IndoEuropeans. We must remember that any date after 6000 BC is Post-IE, and any date after 5000 BC is Post-IE-Proper. From this vantage point it is clear that the expansions of such peoples as the IndoIranians and the Celts were late Post-IE-Proper developments. Celtic en-

thusiasts have made much of a similarity between the early Irish and the Sanskrit speakers in regard to their legal, religious and epic systems, supposing that this is proof that these systems are of (undivided) IndoEuropean date. But this result is based on the highly fallacious assumption that the Celts and the IndoIranians represent respectively Western and Eastern geographical extremes of the primordial IndoEuropean population. On the contrary, it is highly probable that the ancestors of both these peoples remained in the Central European area -- i.e., between the upper bend of the Danube and the San river -- until almost 2000 BC, and in the intervening 3000 years since the Linear expansion there was ample time for Post-IE-Proper ideologies, customs and modes of life to develop in this region and thus to become the common patrimony of both peoples. Such similarities have nothing to do with the undivided IndoEuropeans.

The same is true even of the Satem development. The nature of this phenomenon is such that it could not have occurred until after the Linear deployment was essentially complete, about 4800 BC. On the other hand, the initial process of labiovelar delabialization was sufficiently unitary that I am convinced that it occurred in a restricted region where the dialects were not as yet sharply differentiated and were in partial and temporary isolation from other dialects -- in this sense, the division between Satem and non-Satem is remarkably dichotomous. Thus it seems to have begun before the centrifugal tendencies of the Middle and the Late Neolithic had gotten very far, perhaps no later than 4000 BC. Hence it was apparently a phenomenon of the latter half of the 5th millennium, making it early Post-IE-Proper.

On the other hand, the adoption of horse domestication seems to have been surprisingly late. From Milisauskas' account there seems to be no clear evidence of it until the Bronze Age (except in the Ukraine). It is true that carts are found in the Late Neolithic period as in the Baden culture, but these may have been drawn by oxen instead of horses -- we know that even the Linear people had oxen. If this scenario is correct as the archeological record suggests, it may explain the sudden increase in the centrifugal tendencies of various Post-IE groups. Here I am assuming that domesticated horses were a significant aid in aggressive actions.

Of course I have left many questions unanswered, and many of the answers I do provide are necessarily provisional because of the insufficiency and the ambiguity of the evidence in many cases.

One of the most fascinating of all the questions which I have not addressed is this: In view of their origins in the Early Neolithic, how is it that several of the branches of

Post-IE remained essentially united until their emergence in historical times? For example, all the Slavic dialects seem to have been almost uniform until South Slavic emerged in the writing of Old Church Slavonic. Also, despite the heterogeneous appearance of the early Germanic languages when they were reduced to writing in the 1st and early 2nd millennia AD, they were sufficiently similar that it seems likely that they were mutually intelligible in Caesar's time. Similar remarks, mutatis mutandis, can be made concerning Anatolian, Greek, IndoIranian and perhaps even Celtic. On the other hand, the separation between OscanUmbrian and LatinFaliscan within Italic is obviously old despite a considerable amount of later reconvergence; also, the separation of East Baltic from Old Prussian seems to be ancient. I get the impression that in most of the branches there was a constant process of absorption of incipiently divergent dialects due to repeated encroachments of one tribe upon another. The unity of Germanic, for example, may have been due to an intensive and persistent sea-borne traffic along Baltic shores; its later divergence has been mainly the aftermath of the barbarian expansions of the Christian Era, which resulted in settlements which were so widely separated that the intercommunication among them was insufficient to maintain linguistic unity.

We are now entering a new era in the comparative linguistics of IndoEuropean. Just as a student of the history of the English language cannot ignore its relationship to other Germanic languages, and just as a Germanicist cannot ignore the rest of IndoEuropean, neither can an IndoEuropeanist safely ignore the other families within Nostratic. The time is more than ripe for this fact to be formally and generally accepted. Once we do this, we are able to identify some of those features of IndoEuropean which are truly old.

For example, thanks to the link with Uralic, we can now understand the origin of gender in IndoEuropean. Although it is true that there are also gender systems in other Nostratic families such as Dravidian, AfroAsiatic, the Tungusic branch of Altaic, and even in Etruscan, it is clear that all of these were the results of mutually independent developments; it is true that the system in Etruscan does exhibit superficial similarities to the dichotomy between masculine and feminine in IE, but the differences in detail show it to be independent. This being the case, it is natural to assume that it would be useless to look outside IndoEuropean for enlightenment in regard to the development of gender in IE, and this is quite true so far as the development of the feminine in IE-Proper is concerned. But the dichotomy between neuter and common is such an ancient feature that we can get no hint as to its origin so long as we restrict our attention to purely IndoEuropean phenomena. It is only when we recognize the link with Uralic that we can see how this feature originated in an earlier dichotomy between definite and indefinite marked by the definite-accusative in -m as in Uralic. This is confirmed by the occurrence of the same phenomenon in Etruscan, where the definite-accusative in -n probably evolved from an original -m as occurred independently in several Uralic and IE languages.

Also, a recognition of the more distant connection with EskimoAleut enables us to understand the nature and origin of the neuter r-n declension in IndoEuropean. So far as the oblique-n portion of this phenomenon is concerned, we can see vestiges in Uralic and Altaic as well as IndoEuropean, but only in EskimoAleut is this trait a dominant and productive feature of the declensional system today. And only in EskimoAleut do we observe an (uvular) absolutive ending which corresponds syntactically (and ultimately phonetically) with the -r ending of the impersonal verb and the nominative-accusative of the neuter r-n declension of IndoEuropean; so far as I know, IndoEuropean and EskimoAleut are the only families which have preserved any traces of this feature to historical times. Nevertheless, this evidence, scanty though it is, is enough to show that the neuter r-n declension was inherited from Nostratic. It was originally the dominant mode of declension, still preserved as such in EskimoAleut.

The fragmentary preservation of this feature in IE is poignantly illustrated by the derivation of English "water", where the -r has been leveled throughout the declension and has thus become an indispensable part of the root. Yet cognates in North and East Germanic, and in some other branches of IE, show a leveling and incorporation in favor of the oblique-n stem. This leads us to suspect that this word was originally a member of the neuter r-n class, and this is confirmed when we observe its cognates in Greek and Hittite. The cognates in Uralic languages show no sign of the -n- or -r, but this is not surprising in view of the fact that Uralic has preserved little trace of this paradigm in its nominal declension -- except, of course, when possessive suffixes are attached. [(It seems plausible that the Uralic genitive in -n and the locative in -nae may also preserve vestiges of the oblique-n stem.)]

Similar remarks can be made concerning the derivation of English "fire" and Greek "pyr", in both of which the -r has been leveled and incorporated into the root. But some cognates in North and East Germanic, and also in Old Prussian, show leveling in favor of the -n-. Again we suspect membership in the neuter r-n class, and this is confirmed by the paradigm of the cognate in Hittite. Hittite preserves an internal laryngeal in this word, which also appears in the AfroAsiatic cognate presented by Bomhard in 41.1, p 407 (with suffixed -m). Although no Uralic *cognate has as yet been formally identified, there are a couple of suggestively similar words in Collinder's Uralic list in 1.3, pp 68-69, 1977. These are Finnish poro, "hot ashes", and puhu, "to blow" (with the mouth, as on hot coals). The word seems to have been originally onomatopoeic in Nostratic, referring to the sound of air issuing from the mouth when blowing on hot coals.*

The other IE word for "fire", Latin ignis, shows no sign of ever having been a member of the neuter r-n class. And an apparent cognate in Eskimo, ingne, correspondingly lacks the usual uvular absolutive ending. The nasal character of the velar in the first syllable seems to have been original since the inexplicable variability of the vowel in the IE cognates (ag- in Sanskrit, ug- in Lithuanian and ig- in Latin, which are mutually incompatible) suggests the early loss of some such characteristic in IE, presumably independently in the separate branches.

In summary, it is clear that the linguistic map of Eurasia and surrounding regions has been repeatedly transformed by the constantly recurring expansions of populations. We know this from many historical examples, and there is abundant reason to believe that it was going on throughout prehistoric times as well. By the nature of the process, it was inevitable that in most cases the intrusive populations were encroaching on peoples whose languages were "genetically" related to their own, however distantly. Consequently, it is not in the least surprising that we can still observe vestigial resemblances of a fundamental nature between languages whose speakers have been as distantly separated as Etruscans from Mongols or IndoEuropeans from Eskimos.

It is equally clear that all the Nostratic families have been profoundly transformed by the revolutionary effect of independent changes which have accumulated over a vast period of time, presumably dating from the end of the most recent Ice Age or even before. It is the proper business of comparative linguists to examine these families to try to separate the secondary features from those which are truly primordial, those which date from Nostratic times. Obviously, the resulting reconstruction of undivided Nostratic will never be as detailed or as confidently received as in the case of younger families such as IndoEuropean or AfroAsiatic, but the efforts will prove instructive and must be persistently applied.

It is time to end this. I have done my part. I hope that others will be encouraged to peer into the horizon as well as to gaze upon the ground.

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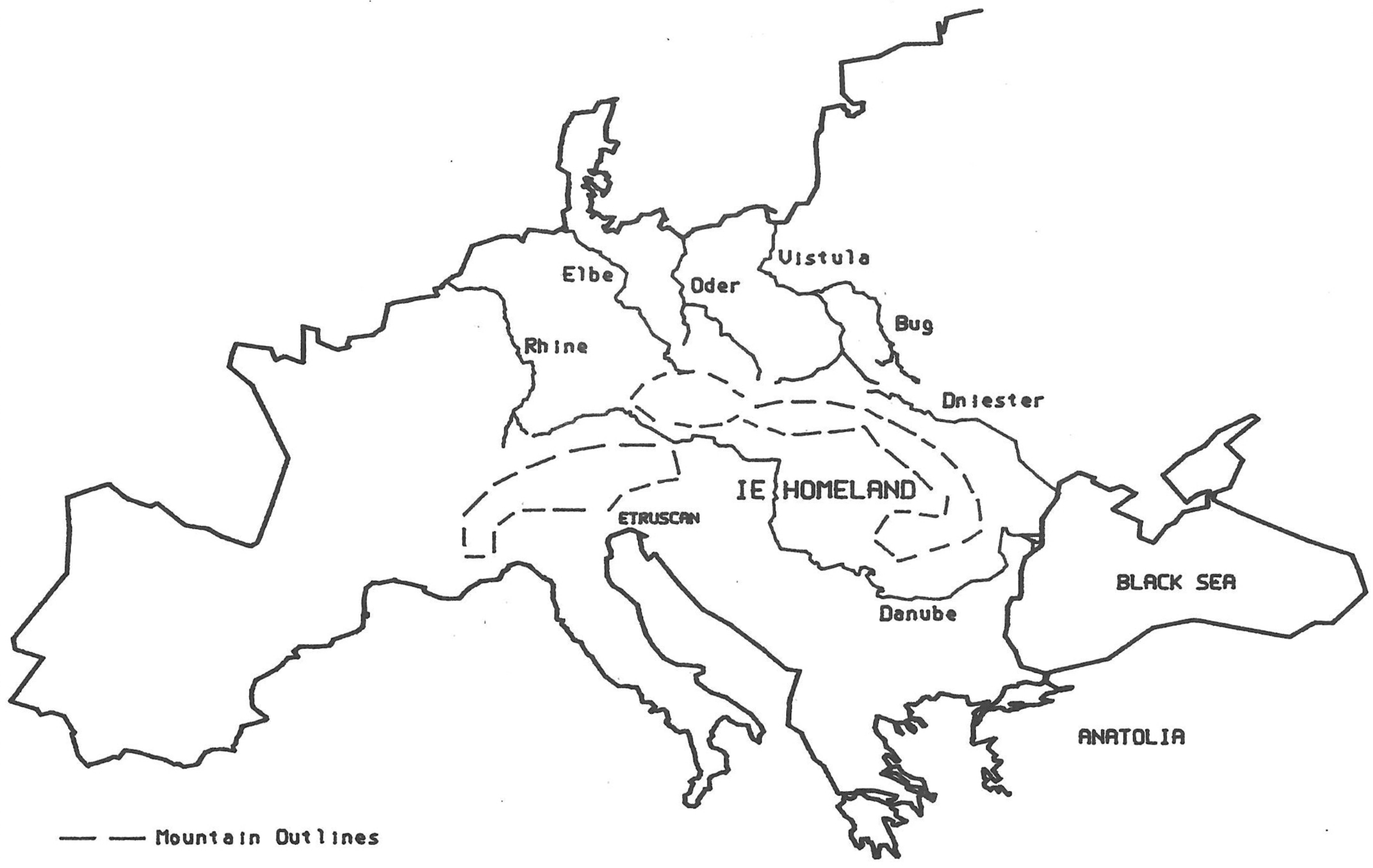
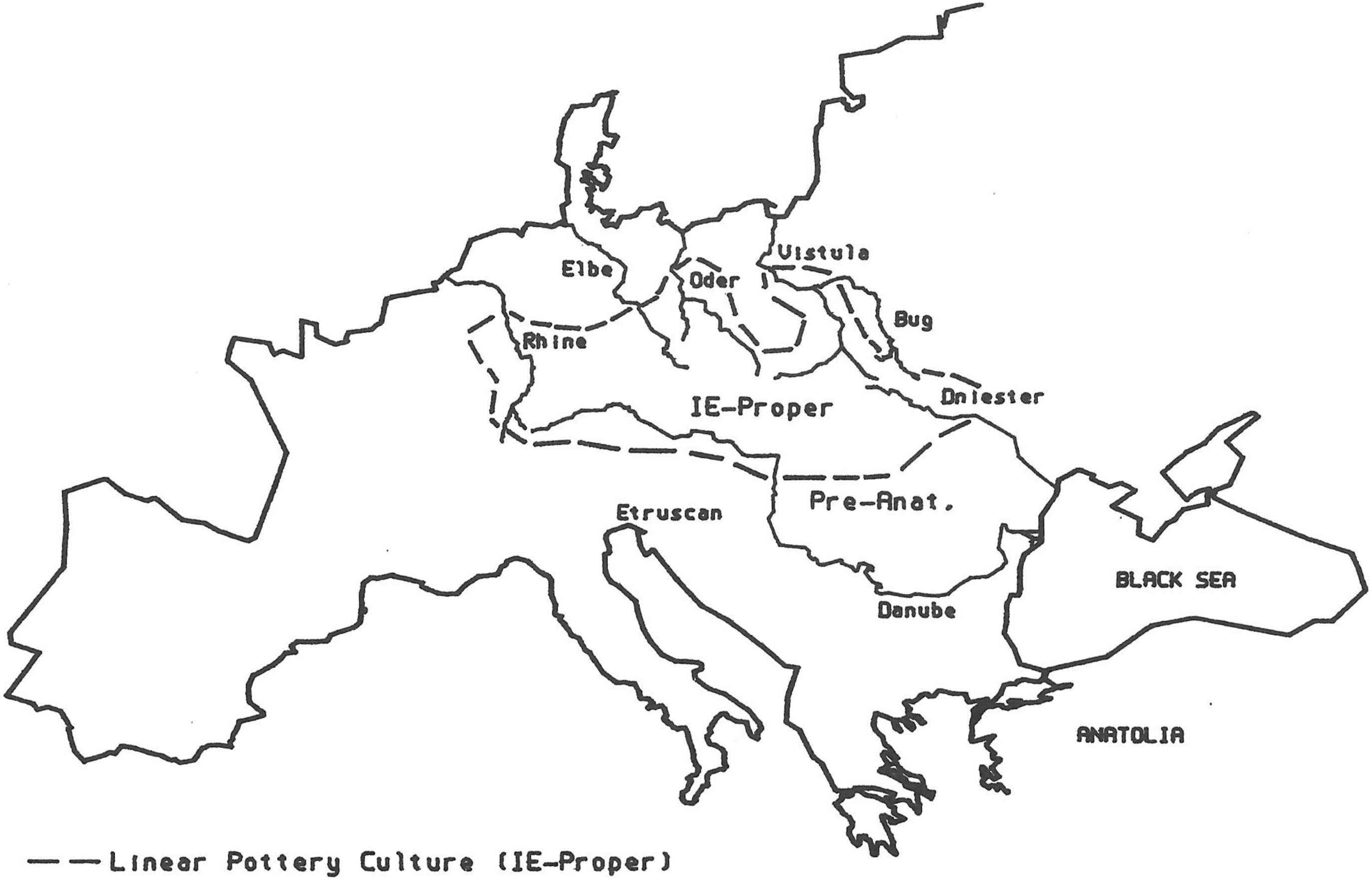
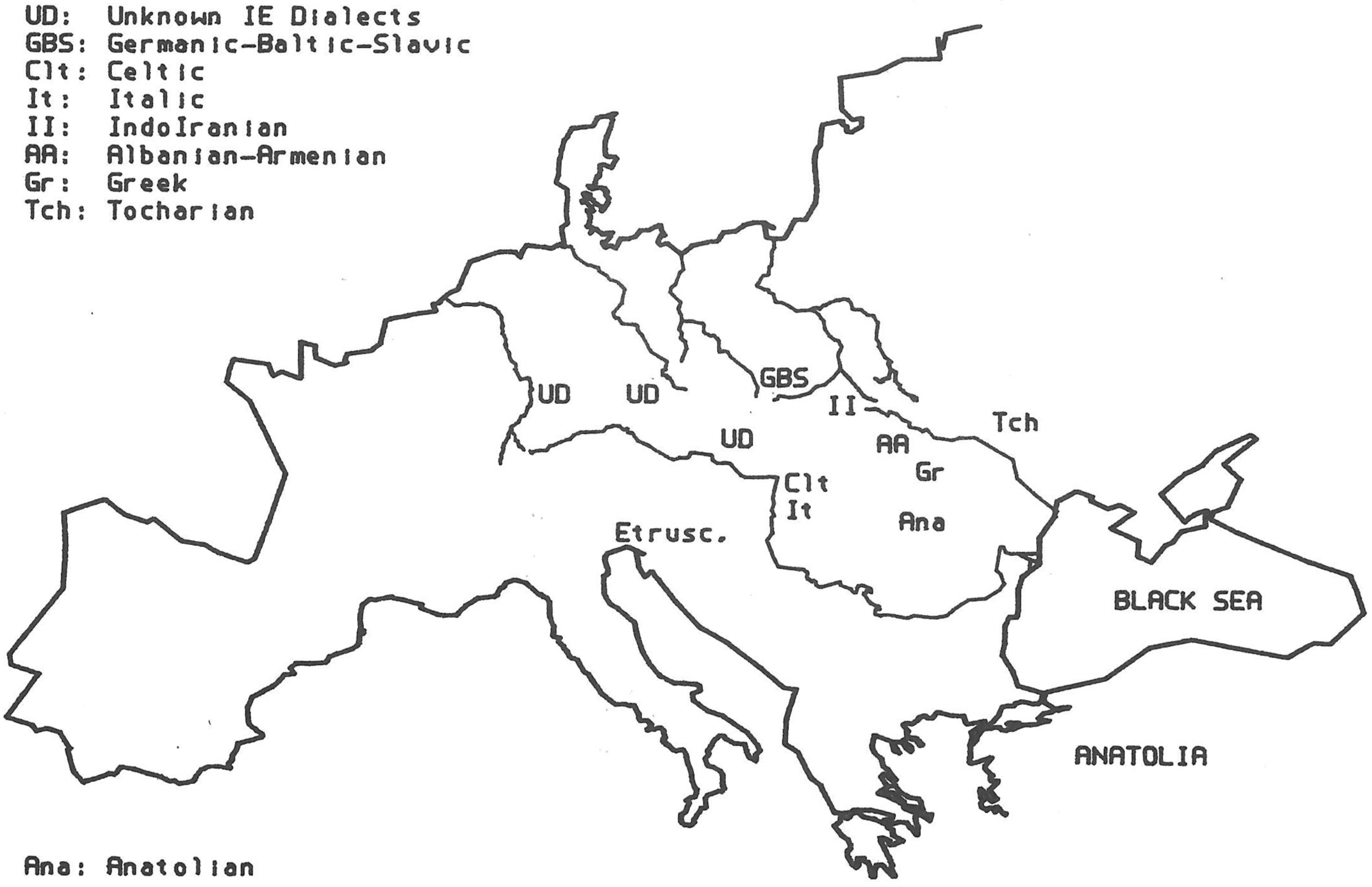


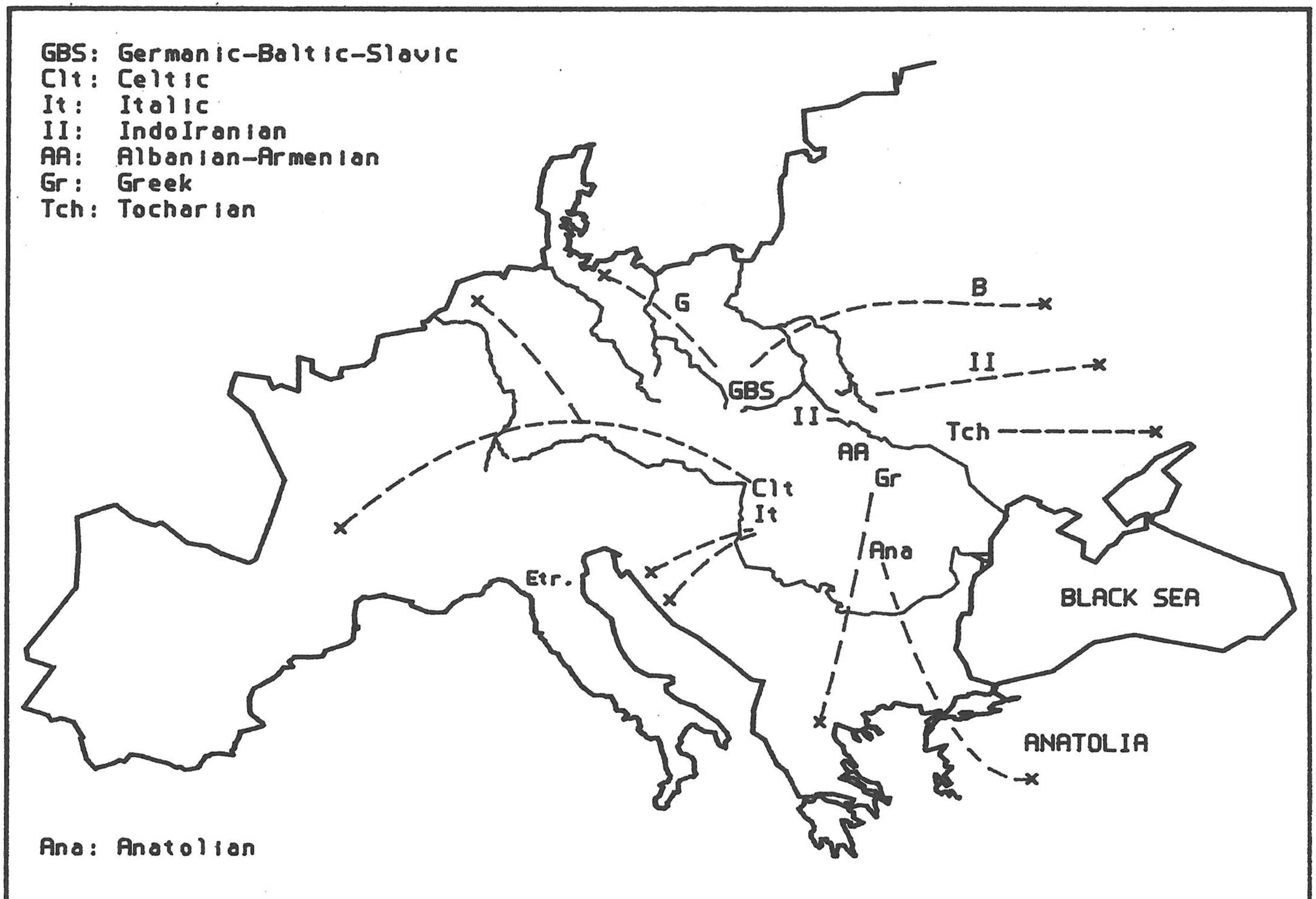
MAP I: Outline of EUROPE with major Rivers, to 5200 BC



MAP II: IE-Proper and Pre-Anatolian, 5-th millennium BC



MAP III: Earliest IE Dialects, to about 4800 BC



MAP IV: Various Post-IE Expansions or Migrations

15. A PERSONAL NOTE

I first became interested in IndoEuropean about half a century ago on my father's farm in Mississippi. In my spare time I would amuse myself by looking up the derivations of words in the family dictionary, and it happened that this book was a treasure in that it provided exceptionally full etymological information. For example, a word of Old English origin would be shown not only in the major Germanic languages, but usually in most of the other branches of IndoEuropean as well -- most of the modern dictionaries are deficient in this respect. I quickly became aware of the importance of discriminating between genuine cognates and borrowed words and of the need for strict adherence to the laws of phonetic correspondence insofar as these may be known. Also, from books saved by my father from his high-school days, and from other books borrowed directly, I taught myself the grammatical elements of German, Latin, Greek, French and Spanish.

In 1940 I enlisted in the United States Army, specializing in radio communications. When it was possible, I would visit libraries to read articles on various linguistic families in current issues of the Encyclopedia Britannica. Toward the end of World War II, in the Philippines, I became briefly acquainted with a cultured Finnish American soldier who cooperated in my elementary investigation of Finnish with amused tolerance. I was aware, of course, that Finnish is not an IE language, so I was surprised to find that it has considerable similarity to the more conservative IE languages in regard to pronominal forms and to details of morphology. As a chauvinistic speaker of an IE language I naturally assumed that these similarities are due to borrowing from one IE language or another.

After the end of the war I obtained a bachelor's degree in electronic communications and spent my working career as a civilian engineer at Wright-Patterson Air Force Base near Dayton and Huber Heights, Ohio, where I now live in retirement. In 1958 I became aware of Collinder's "Fenno-Ugric Vocabulary", which actually could be more accurately entitled "Uralic Vocabulary" since it focuses mostly on identifying those lexical items which can be certified as stemming from Common Uralic. Reading this book, I became aware that the Finnish pronominal forms could not possibly be due to borrowing from IE languages since they are shared by all the other Uralic languages, including Samoyedic -- and Collinder expressly states that the Samoyedic languages show no evidence of borrowing from any IE language, including Common IE. I was further impressed by the fact that most of these forms are found in Altaic languages also. I was beginning to suspect that these three families may have had an early common origin; thus it might become necessary to take the Nostratic hypothesis seriously.

15. A Personal Note

In 1959, on a short vacation visit to Uppsala, I encountered the powerful mind of Prof. Tryggve Skoeld, whose generous help and encouragement are gratefully remembered.

Unfortunately, the increasing pressure of engineering problems prevented me from pursuing the matter further. It was not until several years later that I was inspired by the casual remark by Collinder, referred to in the Foreword herein, which led me to perform the statistical tests which demonstrated that the numbers of pronominal agreements among the three Eurasiatic families are highly significant, implying the existence of a unique historical cause for these phenomena. Further consideration eliminated borrowing as a significant factor, leaving only the hypothesis of common "genetic" descent as a viable suggestion.

This result impelled me to write the pamphlet entitled "The Eurasiatic Pronouns and the Indo-Uralic Question", reprinted here in edited and shortened form. In the course of that writing I came to realize that the common conception of the undivided IndoEuropeans as an egregiously warlike horse-riding people is not supported by any evidence worthy of the name, and that the usual view of them as having conquered much of Europe from Southern Russia or Central Asia is demonstrably false.

I retired from engineering at the end of 1974. This left me free to consult current publications with greater intensity than previously. The result has been that my recently formed views have been confirmed in all major respects; the main difference is that I now feel impelled to regard the concept of "Nostratic" with greater seriousness.

The reason I have expatiated on my personal history is to show that my present views are not the result of a hastily conceived inspiration but have instead been formed gradually during several decades of earnest study.